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THE PSYCHOLOGICAL REVIEW

DRIVES AND THE C.N.S. (CONCEPTUAL NERVOUS SYSTEM)[1]

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The problem of motivation of course lies close to the heart of the general problem of understanding behavior, yet it sometimes seems the least realistically treated topic in the literature. In great part, the difficulty concerns that c.n.s., or "conceptual nervous system," which Skinner disavowed and from whose influence he and others have tried to escape. But the conceptual nervous system of 1930 was evidently like the gin that was being drunk about the same time; it was homemade and none too good, as Skinner pointed out, but it was also habit-forming; and the effort to escape has not really been successful. Prohibition is long past. If we *must* drink we can now get better liquor; likewise, the conceptual nervous system of 1930 is out of date and—if we must neurologize—let us use the best brand of neurology we can find.

Though I personally favor both alcohol and neurologizing, in moderation, the point here does not assume that either is a good thing. The point is that psychology is intoxicating itself with a worse brand than it need use. Many psychologists do not think in terms of neural anatomy; but merely

adhering to certain classical frameworks shows the limiting effect of earlier neurologizing. Bergmann (2) has recently said again that it is logically possible to escape the influence. This does not change the fact that, in practice, it has not been done.

Further, as I read Bergmann, I am not sure that he really thinks, deep down, that we should swear off neurologizing entirely, or at least that we should all do so. He has made a strong case for the functional similarity of intervening variable and hypothetical construct, implying that we are dealing more with differences of degree than of kind. The conclusion *I* draw is that both can properly appear in the same theory, using intervening variables to whatever extent is most profitable (as physics for example does), and conversely not being afraid to use some theoretical conception merely because it might become anatomically identifiable.

For many conceptions, at least, Mac-Corquodale and Meehl's (26) distinction is relative, not absolute; and it must also be observed that physiological psychology makes free use of "dispositional concepts" as well as "existential" ones. Logically, this leaves room for some of us to make more use of explicitly physiological constructs than others, and still lets us stay in communication with one another. It also shows how one's views concerning motivation, for example, might be more

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influenced than one thinks by earlier physiological notions, since it means that an explicitly physiological conception might be restated in words that have—apparently—no physiological reference.

What I propose, therefore, is to look at motivation as it relates to the c.n.s.—or conceptual nervous system—of three different periods: as it was before 1930, as it was say 10 years ago, and as it is today. I hope to persuade you that some of our current troubles with motivation are due to the c.n.s. of an earlier day, and ask that you look with an open mind at the implications of the current one. Today's physiology suggests new psychological ideas, and I would like to persuade you that they make psychological sense, no matter how they originated. They might even provide common ground—not necessarily agreement, but communication, something nearer to agreement—for people whose views at present may seem completely opposed. While writing this paper I found myself having to make a change in my own theoretical position, as you will see, and though you may not adopt the same position you may be willing to take another look at the evidence, and consider its theoretical import anew.

Before going on it is just as well to be explicit about the use of the terms motivation and drive. "Motivation" refers here in a rather general sense to the energizing of behavior, and especially to the sources of energy in a particular set of responses that keep them temporarily dominant over others and account for continuity and direction in behavior. "Drive" is regarded as a more specific conception about the way in which this occurs: a hypothesis of motivation, which makes the energy a function of a special process distinct from those S-R or cognitive functions that are energized. In some contexts,

therefore, "motivation" and "drive" are interchangeable.

MOTIVATION IN THE CLASSICAL (PRE-1930) C.N.S.

The main line of descent of psychological theory, as I have recently tried to show (20), is through associationism and the stimulus-response formulations. Characteristically, stimulus-response theory has treated the animal as more or less inactive unless subjected to special conditions of arousal. These conditions are first, hunger, pain, and sexual excitement; and secondly, stimulation that has become associated with one of these more primitive motivations.

Such views did not originate entirely in the early ideas of nervous function, but certainly were strengthened by them. Early studies of the nerve fiber seemed to show that the cell is inert until something happens to it from outside; therefore, the same would be true of the collection of cells making up the nervous system. From this came the explicit theory of drives. The organism is thought of as like a machine, such as the automobile, in which the steering mechanism—that is, stimulus-response connections—is separate from the power source, or drive. There is, however, this difference: the organism may be endowed with three or more different power plants. Once you start listing separate ones, it is hard to avoid five: hunger, thirst, pain, maternal, and sex drives. By some theorists, these may each be given a low-level steering function also, and indirectly the steering function of drives is much increased by the law of effect. According to the law, habits—steering functions—are acquired only in conjunction with the operation of drives.

Now it is evident that an animal is often active and often learns when there is little or no drive activity of the kinds listed. This fact has been dealt with in

two ways. One is to postulate additional drives—activity, exploratory, manipulatory, and so forth. The other is to postulate acquired or learned drives, which obtain their energy, so to speak, from association with primary drives.

It is important to see the difficulties to be met by this kind of formulation, though it should be said at once that I do not have any decisive refutation of it, and other approaches have their difficulties, too.

First, we may overlook the rather large number of forms of behavior in which motivation cannot be reduced to biological drive plus learning. Such behavior is most evident in higher species, and may be forgotten by those who work only with the rat or with restricted segments of the behavior of dog or cat. (I do not suggest that we put human motivation on a different plane from that of animals [7]; what I am saying is that certain peculiarities of motivation increase with phylogenesis, and though most evident in man can be clearly seen with other higher animals.) What is the drive that produces panic in the chimpanzee at the sight of a model of a human head; or fear in some animals, and vicious aggression in others, at the sight of the anesthetized body of a fellow chimpanzee? What about fear of snakes, or the young chimpanzee's terror at the sight of strangers? One can accept the idea that this is "anxiety," but the anxiety, if so, is not based on a prior association of the stimulus object with pain. With the young chimpanzee reared in the nursery of the Yerkes Laboratories, after separation from the mother at birth, one can be certain that the infant has never seen a snake before, and certainly no one has told him about snakes; and one can be sure that a particular infant has never had the opportunity to associate a strange face with pain. Stimulus generalization does not explain fear of

strangers, for other stimuli in the same class, namely, the regular attendants, are eagerly welcomed by the infant.

Again, what drive shall we postulate to account for the manifold forms of anger in the chimpanzee that do not derive from frustration objectively defined (22)? How account for the petting behavior of young adolescent chimpanzees, which Nissen (36) has shown is independent of primary sex activity? How deal with the behavior of the female who, bearing her first infant, is terrified at the sight of the baby as it drops from the birth canal, runs away, never sees it again after it has been taken to the nursery for rearing; and who yet, on the birth of a *second* infant, promptly picks it up and violently resists any effort to take it from her?

There is a great deal of behavior, in the higher animal especially, that is at the very best difficult to reduce to hunger, pain, sex, and maternal drives, plus learning. Even for the lower animal it has been clear for some time that we must add an exploratory drive (if we are to think in these terms at all), and presumably the motivational phenomena recently studied by Harlow and his colleagues (16, 17, 10) could also be comprised under such a drive by giving it a little broader specification. The curiosity drive of Berlyne (4) and Thompson and Solomon (46), for example, might be considered to cover both investigatory and manipulatory activities on the one hand, and exploratory, on the other. It would also comprehend the "problem-seeking" behavior recently studied by Mahut and Havelka at McGill (unpublished studies). They have shown that the rat which is offered a short, direct path to food, and a longer, variable and indirect pathway involving a search for food, will very frequently prefer the more difficult, but more "interesting" route.

But even with the addition of a curi-

osity-investigatory-manipulatory drive, and even apart from the primates, there is still behavior that presents difficulties. There are the reinforcing effects of incomplete copulation (43) and of saccharin intake (42, 11), which do not reduce to secondary reward. We must not multiply drives beyond reason, and at this point one asks whether there is no alternative to the theory in this form. We come, then, to the conceptual nervous system of 1930 to 1950.

MOTIVATION IN THE C.N.S. OF 1930–1950

About 1930 it began to be evident that the nerve cell is not physiologically inert, does not have to be excited from outside in order to discharge (19, p. 8). The nervous system is alive, and living things by their nature are active. With the demonstration of spontaneous activity in c.n.s. it seemed to me that the conception of a drive system or systems was supererogation.

For reasons I shall come to later, this now appears to me to have been an oversimplification; but in 1945 the only problem of motivation, I thought, was to account for the *direction* taken by behavior. From this point of view, hunger or pain might be peculiarly effective in guiding or channeling activity but not needed for its arousal. It was not surprising, from this point of view, to see human beings liking intellectual work, nor to find evidence that an animal might learn something without pressure of pain or hunger.

The energy of response is not in the stimulus. It comes from the food, water, and oxygen ingested by the animal; and the violence of an epileptic convulsion, when brain cells for whatever reason decide to fire in synchrony, bears witness to what the nervous system can do when it likes. This is like a whole powder magazine exploding at once. Ordinary behavior can be thought of

as produced by an organized series of much smaller explosions, and so a "self-motivating" c.n.s. might still be a very powerfully motivated one. To me, then, it was astonishing that a critic could refer to mine as a "motivationless" psychology. What I had said in short was that any organized process in the brain is a motivated process, inevitably, inescapably; that the human brain is built to be active, and that as long as it is supplied with adequate nutrition will continue to be active. Brain activity is what determines behavior, and so the only behavioral problem becomes that of accounting for *in*activity.

It was in this conceptual frame that the behavioral picture seemed to negate the notion of drive, as a separate energizer of behavior. A pedagogical experiment reported earlier (18) had been very impressive in its indication that the human liking for work is not a rare phenomenon, but general. All of the 600-odd pupils in a city school, ranging from 6 to 15 years of age, were suddenly informed that they need do no work whatever unless they wanted to, that the punishment for being noisy and interrupting others' work was to be sent to the playground to play, and that the reward for being good was to be allowed to do more work. In these circumstances, *all* of the pupils discovered within a day or two that, within limits, they preferred work to no work (and incidentally learned more arithmetic and so forth than in previous years).

The phenomenon of work for its own sake is familiar enough to all of us, when the timing is controlled by the worker himself, when "work" is not defined as referring alone to activity imposed from without. Intellectual work may take the form of trying to understand what Robert Browning was trying to say (if anything), to discover what it is in Dali's paintings that can interest others, or to predict the out-

come of a paperback mystery. We systematically underestimate the human need of intellectual activity, in one form or another, when we overlook the intellectual component in art and in games. Similarly with riddles, puzzles, and the puzzle-like games of strategy such as bridge, chess, and *go;* the frequency with which man has devised such problems for his own solution is a most significant fact concerning human motivation.

It is, however, not necessarily a fact that supports my earlier view, outlined above. It is hard to get these broader aspects of human behavior under laboratory study, and when we do we may expect to have our ideas about them significantly modified. For my views on the problem, this is what has happened with the experiment of Bexton, Heron, and Scott (5). Their work is a long step toward dealing with the realities of motivation in the well-fed, physically comfortable, adult human being, and its results raise a serious difficulty for my own theory. Their subjects were paid handsomely to do nothing, see nothing, hear or touch very little, for 24 hours a day. Primary needs were met, on the whole, very well. The subjects suffered no pain, and were fed on request. It is true that they could not copulate, but at the risk of impugning the virility of Canadian college students I point out that most of them would not have been copulating anyway and were quite used to such long stretches of three or four days without primary sexual satisfaction. The secondary reward, on the other hand, was high: $20 a day plus room and board is more than $7000 a year, far more than a student could earn by other means. The subjects then should be highly motivated to continue the experiment, cheerful and happy to be allowed to contribute to scientific knowledge so painlessly and profitably.

In fact, the subject was well motivated for perhaps four to eight hours, and then became increasingly unhappy. He developed a need for stimulation of almost any kind. In the first preliminary exploration, for example, he was allowed to listen to recorded material on request. Some subjects were given a talk for 6-year-old children on the dangers of alcohol. This might be requested, by a grown-up male college student, 15 to 20 times in a 30-hour period. Others were offered, and asked for repeatedly, a recording of an old stock-market report. The subjects looked forward to being tested, but paradoxically tended to find the tests fatiguing when they did arrive. It is hardly necessary to say that the whole situation was rather hard to take, and one subject, in spite of not being in a special state of primary drive arousal in the experiment but in real need of money outside it, gave up the secondary reward of $20 a day to take up a job at hard labor paying $7 or $8 a day.

This experiment is not cited primarily as a difficulty for drive theory, although three months ago that is how I saw it. It *will* make difficulty for such theory if exploratory drive is not recognized; but we have already seen the necessity, on other grounds, of including a sort of exploratory-curiosity-manipulatory drive, which essentially comes down to a tendency to seek varied stimulation. This would on the whole handle very well the motivational phenomena observed by Heron's group.

Instead, I cite their experiment as making essential trouble for my own treatment of motivation (19) as based on the conceptual nervous system of 1930 to 1945. If the thought process is internally organized and motivated, why should it break down in conditions of perceptual isolation, unless emotional disturbance intervenes? But it did break down when no serious emotional

change was observed, with problem-solving and intelligence-test performance significantly impaired. Why should the subjects themselves report (a) after four or five hours in isolation that they could not follow a connected train of thought, and (b) that their motivation for study or the like was seriously disturbed for 24 hours or more after coming out of isolation? The subjects were reasonably well adjusted, happy, and able to think coherently for the first four or five hours of the experiment; why, according to my theory, should this not continue, and why should the organization of behavior not be promptly restored with restoration of a normal environment?

You will forgive me perhaps if I do not dilate further on my own theoretical difficulties, paralleling those of others, but turn now to the conceptual nervous system of 1954 to ask what psychological values we may extract from it for the theory of motivation. I shall not attempt any clear answer for the difficulties we have considered—the data do not seem yet to justify clear answers—but certain conceptions can be formulated in sufficiently definite form to be a background for new research, and the physiological data contain suggestions that may allow me to retain what was of value in my earlier proposals while bringing them closer to ideas such as Harlow's (16) on one hand and to reinforcement theory on the other.

MOTIVATION AND C.N.S. IN 1954

For psychological purposes there are two major changes in recent ideas of nervous function. One concerns the single cell, the other an "arousal" system in the brain stem. The first I shall pass over briefly; it is very significant, but does not bear quite as directly upon our present problem. Its essence is that there are two kinds of activity in the

nerve cell: the spike potential, or actual firing, and the dendritic potential, which has very different properties. There is now clear evidence (12) that the dendrite has a "slow-burning" activity which is not all-or-none, tends not to be transmitted, and lasts 15 to 30 milliseconds instead of the spike's one millisecond. It facilitates spike activity (23), but often occurs independently and may make up the greater part of the EEG record. It is still true that the brain is always active, but the activity is not always the transmitted kind that conduces to behavior. Finally, there is decisive evidence of primary inhibition in nerve function (25, 14) and of a true fatigue that may last for a matter of minutes instead of milliseconds (6, 9). These facts will have a great effect on the hypotheses of physiological psychology, and sooner or later on psychology in general.

Our more direct concern is with a development to which attention has already been drawn by Lindsley (24): the nonspecific or diffuse projection system of the brain stem, which was shown by Moruzzi and Magoun (34) to be an *arousal* system whose activity in effect makes organized cortical activity possible. Lindsley showed the relevance to the problem of emotion and motivation; what I shall attempt is to extend his treatment, giving more weight to cortical components in arousal. The point of view has also an evident relationship to Duffy's (13).

The arousal system can be thought of as representing a second major pathway by which all sensory excitations reach the cortex, as shown in the upper part of Fig. 1; but there is also feedback from the cortex and I shall urge that the *psychological* evidence further emphasizes the importance of this "downstream" effect.

In the classical conception of sensory function, input to the cortex was via
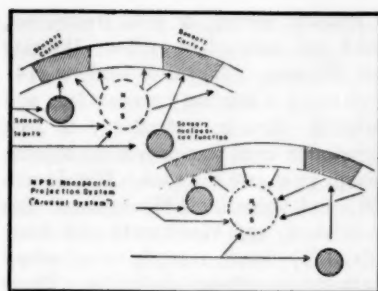
FIG. 1

the great projection systems only: from sensory nerve to sensory tract, thence to the corresponding sensory nucleus of the thalamus, and thence directly to one of the sensory projection areas of the cortex. These are still the direct sensory routes, the quick efficient transmitters of information. The second pathway is slow and inefficient; the excitation, as it were, trickles through a tangled thicket of fibers and synapses, there is a mixing up of messages, and the scrambled messages are delivered indiscriminately to wide cortical areas. In short, they are messages no longer. They serve, instead, to tone up the cortex, with a background supporting action that is completely necessary if the messages proper are to have their effect. Without the arousal system, the sensory impulses by the direct route reach the sensory cortex, but go no farther; the rest of the cortex is unaffected, and thus learned stimulus-response relations are lost. The waking center, which has long been known, is one part of this larger system; any extensive damage to it leaves a permanently inert, comatose animal.

Remember that in all this I am talking conceptual nervous system: making a working simplification, and abstracting for psychological purposes; and all these statements may need qualification, especially since research in this area is moving rapidly. There is reason to think, for example, that the arousal system may not be homogeneous, but may consist of a number of subsystems with distinctive functions (38). Olds and Milner's (37) study, reporting "reward" by direct intracranial stimulation, is not easy to fit into the notion of a single, homogeneous system. Sharpless' (40) results also raise doubt on this point, and it may reasonably be anticipated that arousal will eventually be found to vary qualitatively as well as quantitatively. But in general terms, psychologically, we can now distinguish two quite different effects of a sensory event. One is the *cue function,* guiding behavior; the other, less obvious but no less important, is the *arousal* or *vigilance function.* Without a foundation of arousal, the cue function cannot exist.

And now I propose to you that, whatever you wish to call it, arousal in this sense is synonymous with a general drive state, and the conception of drive therefore assumes anatomical and physiological identity. Let me remind you of what we discussed earlier: the drive is an energizer, but not a guide; an engine but not a steering gear. These are precisely the specifications of activity in the arousal system. Also, learning is dependent on drive, according to drive theory, and this too is applicable in general terms—no arousal, no learning; and efficient learning is possible only in the waking, alert, responsive animal, in which the level of arousal is high.

Thus I find myself obliged to reverse my earlier views and accept the drive conception, not merely on physiological grounds but also on the grounds of some of our current psychological studies. The conception is somewhat modified, but the modifications may not be entirely unacceptable to others.

Consider the relation of the effectiveness of cue function, actual or poten-

Optimal Level of
response and learning

Level of "cue function"
(or possibility thereof)

increasing
alertness,interest,
positive emotion

increasing
emotional
disturbance,
anxiety

Sleep

point of waking

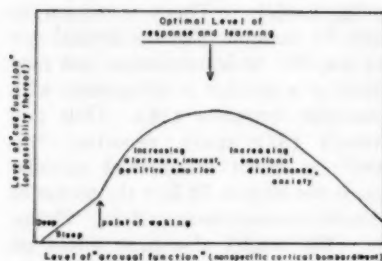Level of "arousal function" (nonspecific cortical bombardment)

FIG. 2

tial, to the level of arousal (Fig. 2). Physiologically, we may assume that cortical synaptic function is facilitated by the diffuse bombardment of the arousal system. When this bombardment is at a low level an increase will tend to strengthen or maintain the concurrent cortical activity; when arousal or drive is at a low level, that is, a response that produces increased stimulation and greater arousal will tend to be repeated. This is represented by the rising curve at the left. But when arousal is at a high level, as at the right, the greater bombardment may interfere with the delicate adjustments involved in cue function, perhaps by facilitating irrelevant responses (a high D arouses conflicting $_sH_R$'s?). Thus there will be an optimal level of arousal for effective behavior, as Schlosberg (39) has suggested. Set aside such physiologizing completely, and we have a significant behavioral conception left, namely, that the same stimulation in mild degree may attract (by prolonging the pattern of response that leads to this stimulation) and in strong degree repel (by disrupting the pattern and facilitating conflicting or alternative responses).

The significance of this relation is in a phenomenon of the greatest importance for understanding motivation in higher animals. This is the *positive attraction of risk taking*, or mild fear, *and*

of problem solving, or mild frustration, which was referred to earlier. Whiting and Mowrer (49) and Berlyne (4) have noted a relation between fear and curiosity—that is, a tendency to seek stimulation from fear-provoking objects, though at a safe distance. Woodworth (50) and Valentine (48) reported this in children, and Woodworth and Marquis (51) have recently emphasized again its importance in adults. There is no doubt that it exists. There is no doubt, either, that problem-solving situations have some attraction for the rat, more for Harlow's (16) monkeys, and far more for man. When you stop to think of it, it is nothing short of extraordinary what trouble people will go to in order to get into more trouble at the bridge table, or on the golf course; and the fascination of the murder story, or thriller, and the newspaper accounts of real-life adventure or tragedy, is no less extraordinary. This taste for excitement *must* not be forgotten when we are dealing with human motivation. It appears that, up to a certain point, threat and puzzle have positive motivating value, beyond that point negative value.

I know this leaves problems. It is not *any* mild threat, *any* form of problem, that is rewarding; we still have to work out the rules for this formulation. Also, I do not mean that there are not secondary rewards of social prestige for risk taking and problem solving—or even primary reward when such behavior is part of lovemaking. But the animal data show that it is not always a matter of extrinsic reward; risk and puzzle can be attractive in themselves, especially for higher animals such as man. If we can accept this, it will no longer be necessary to work out tortuous and improbable ways to explain why human beings work for money, why school children should learn with-

out pain, why a human being in isolation should dislike doing nothing.

One other point before leaving Fig. 2: the low level of the curve to the right. You may be skeptical about such an extreme loss of adaptation, or disturbance of cue function and S-R relations, with high levels of arousal. Emotion is persistently regarded as energizing and organizing (which it certainly is at the lower end of the scale, up to the optimal level). But the "paralysis of terror" and related states do occur. As Brown and Jacobs (8, p. 753) have noted, "the presence of fear may act as an energizer . . . and yet lead in certain instances to an increase in immobility." Twice in the past eight months, while this address was being prepared, the Montreal newspapers reported the behavior of a human being who, suddenly finding himself in extreme danger but with time to escape, simply made no move whatever. One of the two was killed; the other was not, but only because a truck driver chose to wreck his truck and another car instead. Again, it is reported by Marshall (27), in a book that every student of human motivation should read carefully, that in the emotional pressure of battle no more than 15 to 25 per cent of men under attack even fire their rifles, let alone use them efficiently.

Tyhurst's (47) very significant study of behavior in emergency and disaster situations further documents the point. The adult who is told that his apartment house is on fire, or who is threatened by a flash flood, may or may not respond intelligently. In various situations, 12 to 25 per cent did so; an equal number show "states of confusion, paralyzing anxiety, inability to move out of bed, 'hysterical' crying or screaming, and so on." Three-quarters or more show a clear impairment of intelligent behavior, often with aimless and irrelevant movements, rather than (as one

might expect) panic reactions. There seems no doubt: the curve at the right must come down to a low level.

Now back to our main problem: If we tentatively identify a general state of drive with degree of arousal, where does this leave hunger, pain, and sex drives? These may still be anatomically separable, as Stellar (45) has argued, but we might consider instead the possibility that there is just one general drive state that can be aroused in different ways. Stellar's argument does not seem fully convincing. There are certainly regions in the hypothalamus that control eating, for example; but is this a *motivating* mechanism? The very essence of such a conception is that the mechanism in question should energize *other* mechanisms, and Miller, Bailey, and Stevenson (31) have shown that the opposite is true.

But this issue should not be pressed too far, with our present knowledge. I have tried to avoid dogmatism in this presentation in the hope that we might try, for once, to see what we have in common in our views on motivation. One virtue of identifying arousal with drive is that it relates differing views (as well as bringing into the focus of attention data that may otherwise be neglected). The important thing is a clear distinction between cue function and arousal function, and the fact that at low levels an increase of drive intensity may be rewarding, whereas at high levels it is a decrease that rewards. Given this point of view and our assumptions about arousal mechanisms, we see that what Harlow has emphasized is the exteroceptively aroused, but still low-level, drive, with cue function of course directly provided for. In the concept of anxiety, Spence and Brown emphasize the higher-level drive state, especially where there is no guiding cue function that would enable the animal to escape threat. The feedback from

cortical functioning makes intelligible Mowrer's (35) equating anxiety aroused by threat of pain, and anxiety aroused in some way by cognitive processes related to ideas of the self. Solomon and Wynne's (44) results with sympathectomy are also relevant, since we must not neglect the arousal effects of interoceptor activity; and so is clinical anxiety due to metabolic and nutritional disorders, as well as that due to some conflict of cognitive processes.

Obviously these are not explanations that are being discussed, but possible lines of future research; and there is one problem in particular that I would urge should not be forgotten. This is the cortical feedback to the arousal system, in physiological terms: or in psychological terms, the *immediate drive value of cognitive processes*, without intermediary. This is psychologically demonstrable, and *has* been demonstrated repeatedly.

Anyone who is going to talk about acquired drives, or secondary motivation, should first read an old paper by Valentine (48). He showed that with a young child you can easily condition fear of a caterpillar or a furry animal, but cannot condition fear of opera glasses, or a bottle; in other words, the fear of some objects, that seems to be learned, was there, latent, all the time. Miller (29) has noted this possibility but he does not seem to have regarded it very seriously, though he cited a confirmatory experiment by Bregman; for in the same passage he suggests that my own results with chimpanzee fears of certain objects, including strange people, may be dealt with by generalization. But this simply will not do, as Riesen and I noted (21). If you try to work this out, for the infant who is terrified on *first* contact with a stranger, an infant who has never shown such terror before, and who has always responded with eager affection to the only

human beings he has made contact with up to this moment, you will find that this is a purely verbal solution.

Furthermore, as Valentine observed, you cannot postulate that the cause of such fear is simply the strange event, the thing that has never occurred before. For the chimpanzee reared in darkness, the first sight of a human being is of course a strange event, by definition; but fear of strangers does not occur until later, until the chimpanzee has had an opportunity to learn to recognize a few persons. The fear is not "innate" but depends on some sort of cognitive or cortical conflict of learned responses. This is clearest when the baby chimpanzee, who knows and welcomes attendant *A* and attendant *B*, is terrified when he sees *A* wearing *B*'s coat. The role of learning is inescapable in such a case.

The cognitive and learning element may be forgotten in other motivations, too. Even in the food drive, some sort of learning is fundamentally important: Ghent (15) has shown this, Sheffield and Campbell (41) seem in agreement, and so does the work of Miller and his associates (3, 32, 30) on the greater reinforcement value of food by mouth, compared to food by stomach tube. Beach (1) has shown the cortical-and-learning element in sex behavior. Melzack (28) has demonstrated recently that even pain responses involve learning. In Harlow's (16) results, of course, and Montgomery's (33), the cognitive element is obvious.

These cortical or cognitive components in motivation are clearest when we compare the behavior of higher and lower species. Application of a *genuine* comparative method is essential, in the field of motivation as well as of intellectual functions (22). Most disagreements between us have related to so-called "higher" motivations. But the evidence I have discussed today need

not be handled in such a way as to maintain the illusion of a complete separation between our various approaches to the problem. It *is* an illusion, I am convinced; we still have many points of disagreement as to relative emphasis, and as to which of several alternative lines to explore first, but this does not imply fundamental and final opposition. As theorists, we have been steadily coming together in respect of ideational (or representative, or mediating, or cognitive) processes; I believe that the same thing can happen, and is happening, in the field of motivation.

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PROBABILISTIC FUNCTIONING AND THE CLINICAL METHOD [1]

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It is probably true that most non-clinicians believe (with considerable justification) that the clinical method does not meet the criteria of science. This belief is ordinarily founded on the following grounds: The process by which the clinician arrives at a decision is private, quasi-rational, and nonrepeatable. Frequently, the clinician cannot report with confidence exactly how he arrives at a decision. He cannot point at the datum, or the configuration of data, which led to his decision. And, if he could, he would be doing nothing more than providing us with an introspective report. When two clinicians are involved, one introspection is left to stand against another. In brief, clinicians' judgments are a function of a process they cannot trace. It is as if we put our empirical data into a computing machine, the processes of which we did not understand and which frequently produced different results depending on which machine we used and when we used it.

These criticisms are difficult to meet; the effort to remove them has centered around the development of clinical tests. The aim of the test is to produce a retraceable process. However, this movement, while vigorous in a numerical sense—there are lots of tests—cannot quite ignore the skeleton in the closet. That is, in the last analysis, in the clinical situation the value of a test depends

upon its agreement with *some* clinician's judgment. The objectivity of the test is no defense against the clinician's decision. All the test can do is to become a reasonable facsimile of the clinician. And since the clinician stands alone as the ultimate criterion, tests stand or fall through their agreement with a reduction process which remains a mystery.

Since the clinical decision is the ultimate criterion, the final measuring device against which other techniques are evaluated, and since this decision process is a *private* one, it is hardly surprising that there is some question as to whether knowledge is increasing in clinical psychology and psychiatry. Therefore, this paper proposes a change in point of departure.

The plan of the paper is this: First, two methodological issues are discussed —the partition between the observer and the object, and a distinction between two types of reduction bases. Second, these methodological issues are interpreted to fit with behavioral fact in general and the clinical situation in particular. Third, a method of research congruent with these behavioral facts is discussed. Finally, an example of the utility of this analysis is presented.

PARTITION BETWEEN SUBJECT AND OBJECT

Concerning the first of the methodological points, some remarks by Lenzen (6) in connection with physics are quite relevant. Although these remarks may or may not carry significance for psychologists above the level of analogy, they are presented here because they clarify a problem common to psychology

and physics; i.e., interaction between observer and object.

According to Lenzen, the partition [2] between the object and observer shifts according to the intent of the observer. For example, "If a physicist is looking at a pointer on a scale, its status depends on the purpose of the observation. If he is using the instrument to measure an electric current, the pointer is an extension of the observer; the object is the electric current. If the physicist is calibrating his instrument, the pointer is part of the object of observation; the light by which the pointer is seen is then an instrument which belongs to the observer" (6, p. 29). Thus, the partition shifts according to the purpose of the investigation.

Now the same holds true for the clinical method. If the patient is being studied under usual circumstances, the partition stands between the subject being studied and the observer. Clearly the clinician is the observer, the subject or patient is the object being observed. If the clinician is studying a patient by means of a test, the test is an extension of the clinician just as a meter, say, is an extension of the physicist. If the clinician is studying a test, the scoring categories are the object of observation just as a pointer would be the object

for a physicist calibrating a meter. But the clinician almost invariably stands beyond the partition as the observer and in most situations the partition is at the object—the patient.

There is an interesting parallel between difficulties in observation in the clinical situation and in the observation of microphysical entities. Lenzen remarks:

In an observation of a micro-physical quantity there occurs an interaction between object and instrument; the instrument reacts against the object and may produce an unpredictable, finite change in the value of a quantity that is . . . being observed (6, p. 30).

The observations of micro-physics require interpretation in terms of classical concepts but the fundamentally unpredictable, finite effects of the disturbances by the instruments of observation lead to a restriction in the applicability of classical concepts to micro-physical objects. The cognitive partition between object and apparatus is the seat of an indeterminacy which limits theoretical physics to the statistical prediction of the results of classically interpreted experiments (6, p. 31).

The parallel between this situation and the clinical situation is clear. The clinician certainly interacts with the object being observed, and, in principle, "may produce an unpredictable, finite change" in the object. Moreover, the object may produce an "unpredictable, finite change" in the observer.[3]

There are thus two points to be made here. First, in order to understand the interaction between the clinician-observer and patient-object, it is proposed that we shift the partition to a point

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[2] Lenzen explicates the meaning of the term "partition" as follows (6, p. 28): "Tactual perception is an interaction between a body and end organs such as those in the tip of a finger. If one touches a desk with a finger, the partition is between them. An observer, however, may be extended by mechanical devices. Bohr has cited the following example: If one firmly grasps a long stick in one's hand and touches it to a body, the body touched is the object of observation, and the stick is an apparatus that may be viewed as part of the observer. It is a psychological fact that one locates the tactual aspect at the end of the stick, so that the partition is between the body and the end of the stick. If, however, the stick is held loosely in the hand, the stick becomes the perceived object, and the partition is between stick and hand."

[3] In a sense, the clinician when describing a patient gives a report on the changes which happened to himself. Note Lenzen's remark here: "In such observations (interaction between subject and object) it is not possible to control the action of the measuring instrument upon the object, for the instrument cannot be investigated while serving as a means of observation" (6). It appears likely that the clinical psychologist has so infrequently been the subject of investigation because he has been serving primarily "as a means of observation."

beyond the clinician-observer where our observations can take place in a noninteractive fashion. Second, it is proposed that we consider the "cognitive partition between object and [clinician]" to be an indeterminacy relation in a full theoretical sense, and utilize research procedures congruent with this proposition.

Stated otherwise, it is suggested that we consider the clinician not as a *reader* of instruments, as tradition has it, but as an instrument to be analyzed and understood in terms of a probability model. (An example of research following these propositions will be presented later.) We turn now to problems relating to a reduction base in the study of the clinical method.

INTERSUBJECTIVE COMMUNICABILITY

Consider next the problem of (macro-) physical measurement. The physical scientist begins with events which are *both* intersubjectively observable *and communicable*. That is, observers agree about a given event with a high degree of reliability and can readily communicate the reason for their response. Thus, observers of boiling and freezing points can agree not only as to when the liquid boils or freezes, but can point to, can communicate the basis for their decision.

Now consider the situation with regard to behavior. Observers of the state of anger may agree that such a state exists (i.e., high reliability may be achieved), *but* they may not be able to communicate the basis for their decision, or they may have decided on different evidence (i.e., intersubjective communicability is not achieved).

The crucial question here is this: Is noncommunicability[4] merely due to

[4] Perhaps a more nearly correct term here would be "limited intersubjective communicability." In the interests of simplicity the author prefers to risk overstating the case—thus, "noncommunicability."

technical difficulties sooner or later to be surmounted? Or is noncommunicability a direct reflection of behavior, that is, a starting point for the study of behavior rather than a difficulty to be eliminated if possible? Should it actually be the case that noncommunicability is a direct reflection of behavior, then psychologists should cope with it theoretically rather than treat it merely as a technical difficulty. If, on the other hand, noncommunicability is merely due to poor circumstances of measurement, then we must turn to the laboratory where circumstances can be arranged more neatly. Here it will be hoped that communicability can be arranged via operational definition. If this can be done (and it is likely that most psychologists believe it can), then clinicians will have to wait for the technical problems of noncommunicability to be overcome in the laboratory.

It is also likely, however, that many psychologists have misgivings about the rapidity with which this goal is being reached. For it does appear that such a position accepts uncritically the hypothesis that noncommunicability is simply a function of inadequate apparatus, i.e., poor techniques. An equally tenable hypothesis would be that the apparatus is not inadequate, but rather that noncommunicability is a phenomenon to be understood rather than one to be eliminated from study. One would then be faced with the problem of developing a behavior theory and a research methodology appropriate to the problem. We now turn to the theoretical problem, that of considering observer-object interaction and noncommunicability jointly in relation to the theoretical concept of vicarious functioning.

VICARIOUS FUNCTIONING

The notion of noncommunicability can be robbed of its metaphysical air

by consideration of the behavioral fact
of *vicarious functioning*. Almost all
students of behavior are in agreement
with Tolman (8) and Brunswik (2)
that higher organisms may substitute
one form of behavior for another in or-
der to achieve a goal. In the biologi-
cal literature this phenomenon has been
termed *equifinality* (2, p. 17). And
concerning the perception of the envi-
ronment, Brunswik and others have
shown that cues to distance, say, may
substitute for one another (1, p. 48).
This phenomenon has been termed
equipotentiality of cues.[5] Thus, vicari-
ous functioning refers to the *variability*
in what might be termed behavioral
"output" (equifinality) and "input"
(equipotentiality of cues for an organ-
ism).

Now the concepts of noncommunica-
bility and observer-object interaction
may be set in parallel to the concepts
of equifinality and equipotentiality.
Consider the clinical situation. The pa-
tient is trying, say, to achieve a cer-
tain goal. The clinician is attempting
to discover the patient's motive. The
patient subst'-utes one form of behav-
ior for another as he attempts to achieve
his goal (equifinality). The clinician
perceives these behaviors, as they sub-
stitute for one another, as cues which
also substitute for one another (equipo-
tentiality). Because of vicarious func-
tioning, then, the clinician is hard-
pressed to point at, to communicate,
the basis for a decision (except in the
special case where univocal cues are
available). Moreover, the partition be-
tween observer and object becomes in-
distinct for the same reason. *Vicarious
functioning, then, lies at the heart of
the private, quasi-rational nature of the
clinical decision.* (Lenzen's phrase is
certainly applicable here: "The cogni-
tive partition between object and ap-
paratus is the seat of an indeterminacy

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[5] This matter is discussed at length by
Brunswik (2, pp. 16–25).

. . ." [6, p. 31].) Thus, assuming
vicarious functioning (equifinality and
equipotentiality) to occur, noncommuni-
cability and observer-object interaction
are not merely regrettable clinical oc-
currences to turn one's back on, but are
starting points for the analysis of the
clinical method, provided the appropri-
ate research method is available.

REPRESENTATIVE DESIGN

What are the requirements of an
appropriate method? An appropriate
method must permit vicarious function-
ing to take place. It must take equi-
potentiality and equifinality as given,
and it must permit inductive generali-
zations despite them.

Brunswik's development of a meth-
odology which he terms "representative
design" (1) seems to meet these cri-
teria. Representative design is in part
developed upon the concept of vicarious
functioning, and requires that this fact
of behavior not be eliminated (1, p. 23
f., 48 f.; 2, sec. 8).

The *uncritical* observance of the cri-
teria of strict classical design (in con-
trast to representative design) of ex-
periments is, to my mind, a principal
stumbling block to the advance of
clinical psychology. As long as vicari-
ous functioning is ruled out of the ex-
perimental laboratory situation in ac-
cord with the tradition of nomothetic
behaviorism, and as long as the parti-
tion between the clinical observer re-
mains at the object, in accord with the
tradition of clinical psychology, so long
must clinical psychology and experi-
mental psychology remain isolated disci-
plines. However, it might be hypothe-
sized that noncommunicability and ob-
server-object interaction are not merely
technical difficulties but reflections of
vicarious functioning. Accepting this
hypothesis, and shifting the traditional
place of the partition from between the
clinician and object to a point *beyond*
the clinician, will then permit clinician-

patient interaction to be studied—provided the nomothetic ideal is relinquished and the principles of representative design are invoked.

Before turning to our examples it is worth noting some of Brunswik's remarks (2, p. 9) which will serve to emphasize the importance of the separation of subject and object as well as the ambiguities derived from vicarious functioning:

Crucial turns in the history of ideas . . . are sometimes described as "Copernican revolutions." They define a succession of increasingly threatening blows to the pride of the ego; in psychoanalytic terms, the history of science is one of "retreating narcissism," or disentanglement of the objective from the subjective and wishful. Copernicus himself dethroned man's planet as the center of a faraway universe; Darwin dethroned the human species as the absolute master of the animal kingdom; Freud went still further and dethroned the conscious ego as the true representative of our own motivational dynamics. Kant and Gestalt psychology complete the picture by showing the subjectivity of the thing-language. Discovery of an ambiguous rather than univocal relationship between distant regions or variables seems to be at the root of most of such revolutions; the new "schools" protest the respective "constancy hypotheses."

This paper, then, protests the "constancy hypothesis" of the clinician, and the nomothetic bias of the laboratory psychologist.

We now turn to two examples of research which utilize features of representative design and which should serve to clarify the above remarks.

ILLUSTRATIONS

Although our suggestions above indicate that our research should begin with the clinician-patient situation, we have found it simpler to begin with the clinician-test situation. That is, instead of beginning with the clinician "measuring" or interpreting a patient via the interview situation, we found it easier to begin with the situation where the clinician "measures" or interprets the

patient via a test. The principle is the same in both cases, however.

Our first example, then, concerns the situation where the clinician interprets the Rorschach test. (Any other interpretive psychological test could have been used, or an example could be taken from clinical medicine.) An investigation carried out by Todd (7) had as its purpose the study of the clinician as he perceives and responds to cues to the subject's intelligence provided by the Rorschach test. Further, Todd's intention was to carry out this study in the same manner as a perception psychologist working within the framework of representative design might study a subject's perception of size, i.e., when multiple cues to distance are available to the subject.

Think of the situation this way. Analogous to the meter stick for measuring the length of a body, we have a standard intelligence test. Analogous to physical cues for judging the length of the body we have Rorschach responses which can be categorized in various ways. The clinician's task is to estimate IQ from the Rorschach responses, or cues, just as the subject's task in the size constancy experiment is to estimate bodily size from physical cues in the environment.

First, we may ask, how well does the psychologist perform? Obviously, the answer depends on the factor of information, i.e., his level of performance depends on the kind and amount of information we give him.[6] For ten clinical psychologists judging the records of 78 patients, and provided with categorized responses alone, the median correlation coefficient between Rorschach-estimated IQ and Wechsler-Bellevue IQ was + .47. This is better than chance and fairly impressive. A reasonable

[6] The Rorschach test provides two general kinds of material: one consists of the entire protocol which contains the subject's responses verbatim, and the other his categorized responses.
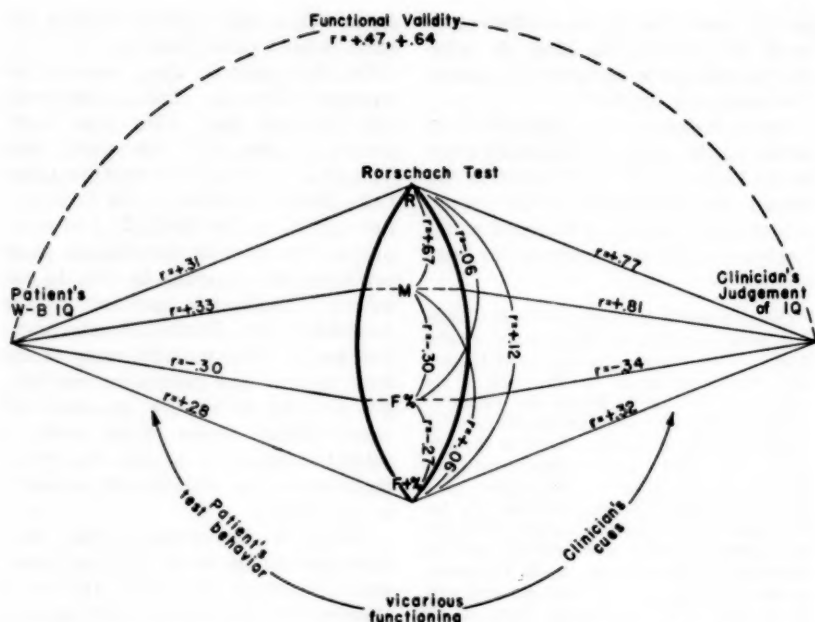
FIG. 1. Functional validity and mediating factors in clinicians' judgments of IQ from the Rorschach test.

question now is, would the addition of the verbal material in the subject's response help or hinder the clinician? Such addition does help. The correlation increases to + .64.[7]

Note that the first step follows our suggestion to set the subject-object partition which Lenzen speaks of at a point between the clinician and the *experimenter* in order that we may study the clinician. In the above example we discover the over-all functional validity of the clinician and Rorschach under two conditions—verbal material present and absent.

Todd next turns to the matter of vicarious functioning—the equifinality of

responses and equipotentiality of cues. First is the matter of the relationship of various Rorschach factors to IQ. Results concerning this question are presented in the left half of Fig. 1. It is clear that Rorschach responses have a hierarchical probability relationship (indicated by the less than perfect correlations between Wechsler-Bellevue IQ and the four Rorschach factors) to Wechsler-Bellevue IQ for this population of subjects whose protocols are the stimulus objects for these clinicians.

The right half of Fig. 1 illustrates the relationship of certain cues to intelligence for the clinician. Here also we find a hierarchical probability relationship between Rorschach cues and clinicians' judgments of IQ.[8] (Intercue re-

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[7] Sophomores in elementary psychology, however, entirely naive with respect to the Rorschach, and estimating IQ on verbal characteristics alone, do practically as well, + .58. Apparently, therefore, training in the use of the Rorschach test adds little to proficiency, at least in this task.

[8] Cf. the data concerning perception and "cue-family hierarchies" presented by Brunswik, as well as his discussion of Hull's "habit-family hierarchy" (2).

lationships between R and M, etc., are also shown in the figure.)

These data are urged as empirical evidence for the concept of vicarious functioning in the clinical situation. They are also urged as evidence for the difficulties of communication under these circumstances.

It will be recalled that we suggested that the clinician be analyzed and understood in terms of a probability model. We now turn to this question: Will the multiple correlation procedure provide a good model for predicting the responses of the clinician? [9] That is, if a multiple regression formula is developed for each clinician, will it be possible to predict his responses (judgment of IQ) to a new set of Rorschach records? In an attempt to answer this question a multiple regression equation (based on the four factors best for each of the ten clinicians) was developed for each of ten clinicians on a sample of 39 Rorschachs, and predictions made for each clinician for a new sample of 39 Rorschach records. The responses predicted for the clinicians correlated quite highly with their actual responses. For the ten clinicians the median r was $+.85$, their individual correlations ranging from $+.74$ to $+.92$. Evidently the multiple correlation model which predicts that the clinician combines the data from the Rorschach in a linear, additive fashion is a good one —it predicts quite successfully in comparison with most psychological efforts.[10]

[9] See Frenkel-Brunswik (3) for the first application of the multiple correlation approach to the analysis of trait-ratings. The reader will recognize the similarity of point of view taken here to that of Kelly and Fiske in reviewing the performance of clinical psychologists in the VA assessment project (5, pp. 200–202).

[10] As an aside, one might ask, as do Kelly and Fiske (5), if the clinicians are as efficient as a multiple correlation procedure? The median correlation between clinicians' judgments of IQ and Wechsler-Bellevue IQ was

Neither Todd nor the author, however, would urge that the latter data be accepted as more than a surprising bit of evidence for the utility of the multiple correlation procedure as a theoretical model. The point is this: given the shift in locus of the partition between subject and object, applying the concept of vicarious functioning in a representative design framework does make it possible to set up *some* probability model to predict the indeterminacy relation suggested earlier.

Note that the design did not disturb the vicarious functioning of either responses or cues. Todd did not, for example, hold all Rorschach responses constant except one, or disrupt their intersubstitutability by arranging them orthogonally as in a systematic factorial design. It is precisely because representative design requires that vicarious functioning of cues be left undisturbed (and thus "representative") that we can specify the scope and precision of our induction for each clinician—such specification being based upon the nature of the sample of Rorschach protocols and the number of protocols in it. (Obviously inductions to *other clinicians* are foregone for sampling reasons.)

From a practical standpoint, it should be mentioned that certain variations were found among the ten clinicians in the effective use of various Rorschach factors. Certain clinicians were found to be using invalid cues, others neglecting valid ones. This kind of practical information should lead to higher predictive validities.

A study by Herring (4) illustrates a further application. Herring studied the problem of clinical psychologists' pre-

$+.470$ (see Fig. 1). A multiple R computed on the four most valid Rorschach factors was $+.479$. Evidently the clinicians are as *efficient* as a multiple correlation procedure. Those factors which were most valid also correlated most highly with the clinicians' judgments.

dictions of patients' responses to surgical anesthesia through the use of psychological tests. Because, according to medical authority, variability in response to surgical anesthesia can best be ascertained through medical clinical judgment, Herring utilized the conception of the clinical method outlined above on *both* sides of the experiment, the predictor side and criterion side.

It may be seen from Fig. 1 that Todd worked only with the vicarious functioning of *psychological* variables. His "true" measure, IQ, was a normative one. Herring, on the other hand, dealt with a more common situation—no criterion available other than another expert's judgment. Ordinarily these two measures (judgments) are correlated, with no attention to the integration process involved on either side. Herring was able, however, through the application of the concept of both physiological vicarious functioning and psychological vicarious functioning, to trace through all these (partial) causal (?) chains so that the network of relationships became clear. That is, he was able to make some progress toward understanding the relationships between (and among) the cues to which the medical clinician responded, the cues to which the clinical psychologist responded, and, therefore *the reasons for correspondence and lack of correspondence between the medical and psychological clinicians' judgments*, i.e., the predictor and criterion variables. For example, Herring was able to show that one of three clinical psychologist's predictions did not correlate with the medical clinicians' criterion judgments because the psychologist's judgments were almost totally a function of one test—which happened to be invalid for this purpose.

SUMMARY

The attempt has been made here to scrutinize the clinical method from a systematic, methodological point of view.

Lenzen's remarks concerning the partition between the subject and object were introduced in order to suggest that the clinician not be considered a reader of instruments, but an instrument to be understood in terms of a probability model. It was suggested that of two criteria for a reduction base, high reliability and communicability, the latter is difficult to achieve, not because of mere technical difficulties but because of a fundamental fact of behavior described as vicarious functioning. Brunswik's "representative design" is asserted to be the research procedure which is congruent with vicarious functioning; its applicability is demonstrated by Todd, who also demonstrated the feasibility of applying a probability model to the clinical situation.

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FEEDBACK THEORY AND THE REFLEX ARC CONCEPT

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In 1896 John Dewey criticized the new reflex arc concept in psychology on a number of grounds and concluded that: ". . . the distinction of sensation and movement as stimulus and response respectively is not a distinction which can be regarded as descriptive of anything which holds of psychical events or existences as such" (1, pp. 369–70).

Dewey used the familiar example of the child reaching for the flame, taken from William James. The theory he severely criticized was the one which simply stated that the sensation of light is a stimulus to the grasping as a response, etc.

It was not until nearly a half-century later that experiments were performed which would allow a testing of the reflex arc concept in gross behavior—eye-hand coordination situations where some continuous record of the position of the "child's hand" and of the "flame" could be made. The experiments which allow for this kind of recording are called tracking experiments. The "flame" is now called the *target;* the "finger tip" becomes a special example of a number of *controls,* including variously loaded joy sticks and handwheels; the field of view, including that which the child sees of his hand, of the candle and of various reference objects, is now called the *display.*

Except for the fact that S is now usually limited to one or two dimensions in which he can move, and that his motivation is directed by instruction rather than by curiosity or burning pain, everything is pretty much the same with today's "human operator" and yesterday's *enfant terrible.* At least the classic example of the reflex arc is no better as an example than is the modern one.

There are but two things left to do to complete the similarity between the two examples. We must restrict the modern tracking to step-function inputs —sudden displacements of the target, in order to make it correspond to the sudden observance (or sudden lighting, if you will) of the candle in the classic example. Further, we shall only concern ourselves with the first half of the Dewey-James example where the child reaches for the flame. This phase illustrates the negative feedback principle to be discussed in this paper.

Tracking of step-function inputs, then, is a good example of the reflex arc. It is also a very special case of the psychophysical method of reproduction where the error contributed by S may eventually be reduced to zero. That is, tracking is the method of reproduction with knowledge of results and usually with continuous recording over time of the target and the control, or at least the difference between them.

Today's tracking, however, usually tends to imply more than a particular experimental situation. There is a theory which goes along with most experiments of this sort: on the one hand it is a more precise statement of the reflex arc notion, and on the other hand it says some quite different things.

Briefly stated, the theory of negative feedback extends to all systems which use a measure of the difference between the target and the control to decrease that difference (3). In the Dewey-James example, the difference between the finger tip and the flame is fed back and used to control perhaps

the velocity of the hand; the greater the distance to go, the faster the movement.

We shall not concern ourselves here with the specific technical advantages of particular methods of analysis of feedback systems or with the special assumptions which many of these methods employ, but will turn instead to the general notion of the feedback system as a model for the reflex arc situation. The purpose of this paper is: first, to ascertain whether or not feedback theory (or servo theory) answers any of the criticisms of reflex arc theory which were raised by Dewey in 1896; and second, to see in what ways feedback theory conflicts with the simple stimulus-response notion implied in the reflex arc theory of Dewey's day.

First of all we notice that by its very nature of being a closed-loop theory, or a theory applying to systems which continually or continuously use a measure of performance to control performance, feedback theory meets Dewey's objection that "[reflex arc] gives us literally an arc, instead of the circuit; and not giving us the circuit of which it is an arc, does not enable us to place, to center, the arc" (1, p. 370). Reflex arc theory, as criticized by Dewey, was apparently an open-loop system. Servo theory closes the loop.

Secondly, Dewey criticized the reflex arc notion as follows:

Upon analysis, we find that we begin not with a sensory stimulus, but with a sensorimotor coordination, the optical-ocular, . . .
Now if this act, the seeing, stimulates another act, the reaching, it is because both of these acts fall within a larger coordination; . . . the ability of the hand to do its work will depend, either directly or indirectly upon its control, as well as its stimulation, by the act of vision . . . The reaching, in turn, must both stimulate and control the seeing . . . (1, p. 358).

Since the analysis of a feedback system is in no way vitiated if the system is made up of a number of subsystems,

it would seem that modern servo theory again fills the bill.

Thirdly, Dewey stated:

The sensation or conscious stimulus is not a thing or existence by itself; it is that phase of a coordination requiring attention because, by reason of the conflict within the coordination it is uncertain how to complete it. . . . The end to follow is, in this sense, the stimulus. . . . From this point of view the discovery of the stimulus is the "response" to possible movement as "stimulus." . . . Generalized, sensation as stimulus, is always that phase of activity requiring to be defined in order that a coordination may be completed (1, p. 368).

This criticism of the reflex arc strikes at the heart of the matter, for it challenges the validity of the stimulus and response as separate entities which are causally related in some way.

We may expand upon this argument by means of a simple example from tracking:
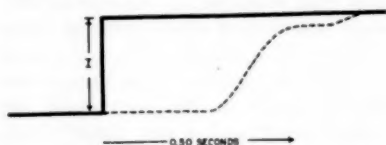
Fig. 1. Diagram of typical step-function input and output. The solid line is the input presented to S through a narrow slit at a fast rate. The dotted line represents S's attempt to keep his control pencil on the input. The error in initial response may be an undershoot (as shown), an overshoot, or zero.

Figure 1 shows a diagram of a typical step-function input (solid line) and output (dotted line). What was presented to S was a dot remaining motionless at one position on the field, and then its instantaneous displacement (I) to another position on the field. The dotted line shows S's attempt to follow this displacement of the target with his own control. His control in this case was a pencil which he attempted to keep on a short segment of line (the solid line), which he viewed through a narrow slit.

His output was then recorded, superimposed upon the input (which E programmed for him by drawing lines at various positions and of various lengths, corresponding to time intervals, on a strip of paper which was fed past the slit where S's pencil point was resting).

This tracking input-output diagram might be taken as a slightly simpler example of the reflex arc situation than the child-candle.

In order better to understand Dewey's objection to the naive stimulus-response description of this type of behavior, we might attempt to apply stimulus response notation to the input-output diagram of tracking. Let us define the *stimulus* as a change in environment over time produced and measured by E by operations which he describes in such a way that they may be duplicated by others, and: (*a*) which, at any time, might provide some or all of the information leading to the response at that time, or (*b*) which, at any time, might possibly determine all or part of the response at that time, or (*c*) to which, at any time, the response might be related in some way, or (*d*) which is a necessary condition for the response.

Now let us check through the characteristics of the input to find what meets the criteria listed above.

First, the mere *existence of the target dot* somewhere (anywhere) on the field cannot alone be called stimulus. The existence of the target dot is not a *change* in environment over time; that is, the dot may always be there during the experiment. Furthermore, it meets none of the other criteria. The existence of the target dot might be considered a necessary condition for the response were it not for the fact that under certain conditions (after the establishment of expectancies of various kinds) the response is obtained without the existence of the target dot (5). At any rate the existence of the target provides no information for the response, cannot possibly determine the response, and the response is not much related to the mere existence of the target.

Second, the *position of the dot relative to the rest of the field* (slit) is clearly not the stimulus. In the first place, we discover that the same response may be obtained to targets positioned anywhere on the field. In the second place, the position of the dot on the field can in no way determine the extent of movement of S, nor provide information to determine the extent of movement. The S has to know "which way" and "how far" before he moves, and the position of the dot relative to the field does not tell him this. The position of the dot on the field (or indeed, the position of the dot relative to any frame of reference of E's) is not related to the response of S within a wide range of conditions.

Third, the *displacement of the target* (the size of the movement I) cannot be called the stimulus since, if it were, we would be restricted only to the condition shown where S's position is equivalent to the position of the target just as displacement occurs. The displacement of the target, like the position of the target, does not tell S "which way" or "how far" except for the time right after the target moves and just before S moves. After S has started to move (either in the right or the wrong direction), I can only be misleading information telling him "how far" or "which way."

The result of all this rather simple reasoning (not to be confused with Dewey's more eloquent generalizations) is that we come to the conclusion that there is nothing about what E does to the environment which can alone be called the stimulus if we give the term stimulus any psychological relevance.

Let us take a more functional view of the situation in an attempt to discover

what might be the "stimulus" as far as S is concerned.

What must S know in order to be able to respond adequately? We gave the answer above when we said that he must know "which way" and "how far." In order to know these, he must know "where he is" and where he "wants to be." But the difference between where he is and where he wants to be, including sign, is the answer to the questions "which way" and "how far." To the extent, then, that we can assume that S's frame of reference relative to which he knows these things corresponds in important ways to the one E gave him (the slit), and furthermore to the degree that where S wants to be corresponds to where E wants him to be, we can say that the stimulus is equal to the "just past" difference between the dotted line and the solid line. Or, to paraphrase the statement made earlier, the difference between where S is and where he wants to be is continually used as the "stimulus" to decrease that difference. If we know where S is (in the same regard as he knows it) and if we can assume that where S wants to be is equivalent to the position of the target relative to that regard, then, and only then can we specify what the "stimulus" is. But notice this about the "stimulus" which we have thus deduced. This "feedback" stimulus is much closer to Dewey's idea of the stimulus than it is to the stimulus response notion which demands that the stimulus be defined a priori in terms of E's frame of reference. The Dewey feedback stimulus is far from a given, constant thing. It is an assumption—both on E's part and upon S's part. It is a continually changing thing. It depends upon where S is and S is moving. The S alters (and at times creates) the stimulus in just as real a sense as does E. Furthermore, the very existence of the stimulus depends upon a "common ground" or frame of reference between E and S and upon common purposes between them. Both the child and the man who lights the candle are parties to a transaction which has no meaning in terms of either one separately.

The "real" stimulus in the reflex arc is the error signal (difference between desired state and present state) and it is not the input defined in terms of E's frame of reference.

This statement is all right as far as it goes but it does not go far enough. The error signal, as E knows it, *is* the difference between the dotted line and the solid line. But S has at his command, of course, *only an estimate* of this difference. There are threshold properties to be considered and there are constant and variable errors. Furthermore, there is much good evidence to show that S's estimate of how far and which way he has to go is influenced to some extent by his past experience: there are serial order effects and "range" effects (2). Perhaps other more complex dependencies may affect S's estimate, especially at high speeds of performance (6).[1] Are we to include all this in the term stimulus? If we are to give the term *any* a priori psychological relevance we must consider *some* characteristics of S in our definition. If we do not want to be arbitrary about it, we are forced to qualify our definition of the term "stimulus" with the following clauses:

1. It is an abstraction out of a process, called, by Dewey, a transaction.

2. It is created by S as well as by E.

3. It is never fixed or constant unless both E and S want it to be.

4. It is an assumption.

5. It is at least a difference between two estimates—one with regard to the

[1] The difference which exists between compensatory and follow tracking indicates that S is using more than the error signal upon which to base his response.

control and one with regard to the target.

6. Experience and various psychological errors enter into this estimate.

We may avoid the above argument by defining stimulus purely in physical terms, that is, relative to E's frame of reference. We may define it as the existence, or the position, or the displacement, or the brightness of the target or as all of these. But if we do this—i.e., define stimulus as independent of S, it is going to have *no* a priori psychological relevance.

For purposes of clarity, we should have a word to stand for the heavy line in Fig. 1. We shall call this the input (input stimulus if you like), understanding that we can never be sure what, if anything, it has been put *into*. The dotted line in Fig. 1 we call the output. When we use these words, we should remember that these are not psychological variables. Nor are they physical variables which are usually relevant to a psychological understanding of this transaction. The input is defined as what E does to the target, relative to the frame of reference he chooses. This is to give input an operational definition relative to his purposes as an experimenter. The output, while it can be defined relative to E's frame of reference, can only have existence for S relative to S's purposes. Only in the event that S's and E's purposes overlap, that they have some goals in common, is an experiment possible.

Now, what is true for the "real" stimulus (as opposed to the "irrelevant" stimulus described above)—namely, that defining it depends upon common purposes between E and S, upon common frames of reference, that it can be created and altered by both E and S, and that it is an abstraction out of a complex transaction—is likewise true for the response.

CONCLUSION

We have taken one of the simplest examples of the reflex arc and attempted to apply stimulus-response concepts to it. In trying to do this we have discovered that we do not know what the stimulus is unless we know what the response is and what previous stimuli and responses were and that, as a matter of fact, we need quite a good understanding of the transaction in order usefully to call anything the stimulus. The same line of argument can be used for the so-called response side. We do not know what the response is unless we know what the stimulus is. Feedback theory tells us that the stimulus is at least the difference between the desired state and the present state. Our knowledge of psychology tells us that in this sense it must be a good deal more.

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INTERPERSONAL BEHAVIOR AS INFLUENCED BY
ACCURACY OF SOCIAL PERCEPTION

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This paper presents a critical examination of two propositions which link accurate social perception with competence in interpersonal behavior and with group efficiency. The first of these propositions maintains that the more knowledge an individual has concerning the intentions, preferences, and beliefs of other persons, the more effectively he can participate in group activity with those other persons. This proposition provides the rationale for much of the training we give to teachers, social workers, clinical psychologists, and others whose work involves continuing interaction with people.

Attempts to verify this proposition have ordinarily involved the use of high sociometric status or high sociometrically evaluated productivity as indexes of the individual's interpersonal competence. With these indexes as criteria, considerable empirical evidence has been produced in support of this proposition. Chowdhry and Newcomb (3) found that persons who were sociometrically chosen, by various criteria, tended to make the most accurate estimates of group opinion on topics relevant to the group's aims and functions. Gage (8) found that high school seniors who did the most accurate job of predicting the responses which others would make to the Kuder Preference Record also received a large percentage of their classmates' sociometric choices. Greer, Galanter, and Nordlie (10) have reported that sociometrically popular members of infantry squads are more accurate than less popular members in predicting the sociometric positions of men in their squads. Norman (17)

analyzed data obtained by a Veterans Administration research project and found that graduate students who were most often rejected by their classmates had the least realistic perceptions of others. Other investigators (2, 4, 9, 21) have also reported positive relationships between accuracy of social perception and interpersonal competence.

The second proposition to be examined here involves an extension of the first. It maintains that groups composed of individuals with accurate social perceptions will be more efficient than groups composed of members with less accurate social perceptions. Faith in this proposition guides much of our marriage counseling and has sometimes been a factor in the selection of work crews and play groups. Much of the empirical evidence in support of this proposition has been provided by studies which were not directly concerned with group efficiency. Thus, Festinger and his associates (6) have frequently given group members erroneous impressions of one another and have found that such impressions can lead to restricted communication within the group, rejection of members, and to lowered group cohesiveness. Such findings strongly suggest that inaccurate social perception can reduce the efficiency with which groups achieve either individual or group goals. Similar indirect support for the proposition is provided in a study by Dymond (5). She found that members of happily married couples (presumably groups which are efficient in achieving individual and group goals) made fewer errors than did members of unhappily married couples in predicting

one another's responses to the Minnesota Multiphasic Personality Inventory. More direct support is provided by Cottrell and Dymond (4), whose ratings of group efficiency were positively correlated with the average accuracy of group members in predicting one another's self-ratings. Greer, Galanter, and Nordlie (10) have also provided direct support for the proposition. They found that infantry squads consisting of men who did a good job predicting one another's sociometric positions within the group were rated more highly than other squads on performance of field maneuvers.

Although the studies cited above provide some basis for confidence in each of the two propositions, other studies have failed to produce positive findings. Thus, the first proposition appears to be contradicted by Campbell's (18) failure to obtain a positive relationship between the accuracy with which naval officers judged the attitudes of their men and the officers' popularity with the crew. Similarly, Sprunger (20) failed to obtain a positive correlation between the accuracy with which office supervisors perceived their subordinates and the popularity of those supervisors with their workers. Perhaps the most notable failure to confirm the second proposition is contained in the work of Fiedler, who has studied the perceptual accuracy of members of efficient and inefficient basketball and surveying teams. His findings with respect to the effect of perceptual accuracy have been generally negative.[1] Indeed, there has sometimes appeared to be a slight association of perceptual accuracy with ineffective performance.

The existence of contradictory findings suggests that the two propositions are neither completely true nor completely false. It is probable that each is a true statement of a relationship which exists under certain limiting conditions, but a false statement when those limiting conditions do not prevail. If this is the case, it is desirable that the limiting conditions be identified and that the propositions be altered to take them into account.

Such limiting conditions might be identified by either of two methods. One method calls for a systematic and comprehensive analysis of the studies which have produced positive findings, and a comparison of those studies with others which have produced negative findings. Although in principle this method should be capable of identifying some of the conditions which determine the validity of the propositions, it is a procedure which involves all of the practical difficulties encountered in any ex post facto analysis. The other method seeks to identify the limiting conditions through an examination of the assumptions which are implicit in the two propositions. These implicit assumptions may themselves specify some of the conditions under which the two propositions are likely to be valid.

This paper will employ the second method in an attempt to identify certain of the conditions which must exist in order that accurate social perception can lead to increased interpersonal competence and group efficiency.

ASSUMPTIONS CONCERNING COLLECTIVE ACTION

The term "collective action" will be used in this paper to designate the behaviors of two or more persons who are attempting to satisfy needs or attain goals through copoerative effort in a "face-to-face" situation. Certain assumptions concerning the nature of collective action are implicit in the propositions cited above. It is implied that efficient collective action is accomplished

[1] Fiedler, F. E. Personal communication, 1954. Fiedler (7) has recently summarized other findings of these studies.

through a dyadic process in which each participant adjusts his own behavior in response to the intentions and preferences of others. This view of collective action has received wide support in the literature. Mead (14) has contended that integrated collective action requires that each individual "take the role of the other," a process by which he gains insight into others' intentions and preferences. Asch (1) has eloquently defended a theory of human interaction in which the existence of mutually shared psychological fields and the capacity for "taking into account the emotions and thoughts of others" provide the basis for coordinated group behavior. In a recent paper Newcomb (16) has pictured the communication process as one in which two or more participating individuals seek "simultaneous orientation" to one another and to the events round about them. Such simultaneous orientation may be described as a relationship in which each individual takes cognizance of, and adjusts to, the attitudes of others. Newcomb says this relationship is essential to human life. Sears (19) has contended that "a dyadic unit can be derived from the combining of two or more monadic units" by taking into account the expectancies which one individual has of another's behavior. It is these expectancies which are "responsible for maintaining the stability of the dyadic unit." Hebb and Thompson (11) have proposed a similar explanation of collective action. Because of man's ability to anticipate not only his own next act and its effect, but also those of others, humans are capable of engaging in a kind of teamwork which is definitely lacking among other mammals.

It is easy to agree with these writers; efficient collective action often does require that each participant anticipate the behavior of others. If individuals engage in role taking, maintain simul-taneous orientations, or have mutually shared fields, their collectively produced behavior sequences may constitute what Sears (19) has called a "dyadic system." Group behavior can become "more than a fortuitously useful conjunction of individual actions" or a mere piling up of "parallel monadic sequences." It can be interlocking behavior which leads to the achievement of individual or group goals with a minimum of wasted effort or needless duplication of behavior. However, there are reasons for doubting that this conception of collective action is universally valid.

Basic to this conception of collective action is the assumption that individuals are relatively free agents who are permitted to alter their own behaviors in response to their perceptions of the intentions or preferences of others. Furthermore, it is assumed that the changes which such perceptions produce in the individual's behaviors will lead to a more thoroughly integrated dyadic system. Unless these assumptions are met, accurate social perception, whether it be achieved through role taking, simultaneous orientation, or shared psychological fields, can scarcely be expected to increase interpersonal competence or group efficiency. In the remainder of this paper it will be contended that much of our most efficient collective action occurs when neither of these assumptions is met, and under circumstances which make accurate social perception relatively unimportant.

Even when these two assumptions are met, accurate social perception may fail to permit or encourage efficient collective action. It should be expected to do so only when the attributes, preferences, or intentions which are accurately perceived are relevant to the task at hand. Accurate perception of irrelevant qualities in the other person may only prove to be distracting in its effect. In

the remainder of this paper "accurate social perception" will refer to the perception of qualities, preferences, and intentions which are relevant to the activities of the group.

ROLE SYSTEMS AND SOCIAL PERCEPTION

Role systems often permit or compel individuals to produce efficient dyadic behavior sequences even though they know little about one another's intentions or preferences. This contention will be clarified by the construction of a model with which to represent some of the characteristics of a role system.

It will be helpful to imagine a matrix of columns and rows, creating a system of cells. Each of the columns will bear the name of a category of persons who are recognized as functionally distinct by the members of the collectivity. Such categories may fall along any of a number of dimensions like age, sex, material wealth, occupation, etc. It is not at all necessary that the categories be unidimensional, and in the case of most collectivities they will not be. It is only necessary that the members of the collectivity be capable of distinguishing between the people who belong in different categories. This requirement is easily met, for if people are not naturally distinguishable they may be made artificially distinguishable through the use of special clothing, insignia, spatial arrangements, and a variety of other means.

The rows in the matrix will each carry the name of a category of situations. Although groups may seldom categorize situations as explicitly as they categorize persons, the multiplicity of stimulus situations precludes the possibility that each can be responded to as something totally unique. As Merton (15) has pointed out, groups tend to evolve standard ways of classifying the situations which are most frequently encountered in collective action. Similar situations tend to be classified into the same category and to be treated as though they were equivalent. Here, as in the case of categories of persons, there need be no apparent logic to the system. It is only necessary that participants be capable of assigning each situation to its proper niche. Standard assignment of situations to their appropriate categories can be achieved through a variety of socialization and indoctrination procedures.

Into the cells which are created by the intersection of columns and rows will be placed the designation of the special behavior which is enforced by the collectivity. Thus, if the collectivity is a symphony orchestra, the cell created by the intersection of the column "first violinist" and the row "third bar, Brahms' Lullaby" might carry instructions to produce specific musical tones at a specific tempo. Not all cells will receive unique entries, and some cells may receive no entry at all because no special behavior is required of a category of persons in the situation represented by a row in the matrix.

In terms of this model, a role consists of all those behaviors which are in the cells of a single column of a complete matrix. In the case of a symphony orchestra, the role of each participant is ordinarily described in detail by the musical score and each musician will know his own role though he may have only a limited understanding of other persons' roles. The role system is represented by the entire matrix and includes the role of each participant. It constitutes a system because the various roles are interlocked in a way which produces highly integrated collective action. The behaviors expected of one person supplement those expected of others. Often, as in the case of a work group, the role of one participant cannot be enacted unless others are enacting their roles properly, and the inade-

quate performance of any one role may disrupt the entire system.

It is the advantage of role systems that behavior synthesis has been incorporated into the system itself, and that participants need not infer the strategies of their associates or improvise an effective synthesis as they engage in collective action. They need only to produce the behaviors which are situationally appropriate for members of their categories. To be sure, other people and their behaviors will often be important elements in the situations to which the individual must respond. But this ordinarily means only that each individual must take into account the overt behavior of other categories of persons; it does not mean that he must make inferences concerning the idiosyncratic intentions or preferences of *unique* individuals. Although participants may develop enduring expectations concerning the unique qualities of specific other individuals, these expectations are not part of the role system. Indeed, participants in a role system are often required to ignore unique qualities and to treat one another as though each were a standard representative of a whole category. The individual who accurately projects himself into the unique psychological fields of others is likely to find it difficult to perform as a standard person or to treat others as such. And as Merton (15) has observed, any deviation from standard performance or standard treatment of others is likely to disrupt the whole role system. Consequently, those individuals who deviate from standard practice are likely to be ostracized. Given the existence of a role system, perceptual accuracy which goes beyond that required for correct categorization of others may decrease group efficiency and reduce interpersonal competence. In this connection it is instructive to recall that Hemphill (12) found that members of large groups (30 or more persons) tended to prefer leaders who did not treat them as unique individuals.

A large proportion of all collective action must be presumed to occur within the framework of role systems where the intentions and preferences of other persons may be largely irrelevant and where the individual's freedom to adjust to such intentions and preferences is, in any case, highly restricted. Naval crews, office staffs, and surveying teams are probable examples of groups which do have elaborate role systems. It is to be noted that investigations of such groups have generally failed to obtain a positive relationship between accuracy of social perception and group efficiency or interpersonal competence. A possible contradiction of this trend is represented by the study of infantry squads (10) in which accuracy in predicting the sociometric status of squad members correlated with group efficiency. However, accuracy in perceiving a status hierarchy is a particular type of accuracy which may be a by-product of efficient use of a role system. If, as may be expected, the efficiency of a squad is a function of the degree to which its members adhere to a role system, then efficient squads will be those which demand that members enact specified roles, and deviation from standard role performance will serve as a major reason for rejection of members. Members of efficient squads would then tend to make accurate predictions of one another's positions in the status hierarchy because each will be aware of the major criterion which has guided the creation of that hierarchy. In squads where adherence to the role system is less highly stressed, efficiency should be lower and members should have more difficulty predicting the number of sociometric choices each man will receive. Until the correctness of this explanation

is ascertained, it is probably unwise to regard this study as a clear contradiction of what appears to be a general trend.

Although the published reports of research in this area are not sufficiently detailed to permit categorical statements, the reported findings seem to support the contention that efficiency in groups which operate within the framework of a role system is not increased by accurate understanding of others' preferences and intentions. What are needed here are controlled studies in which groups with and without the benefit of a role system are confronted by situations requiring coordinated action. If role systems do, in fact, have the properties described above, accuracy of interpersonal understanding should have no relationship (or perhaps a negative relationship) to the efficiency of groups which have appropriate role systems. On the other hand, there should be a positive relationship between the perceptual accuracy and the efficiency of groups without role systems, assuming, of course, that members are motivated to cooperate.[2]

SUMMARY

This paper has examined two propositions which maintain that accurate social perception is responsible for increased interpersonal competence and group efficiency. Although such propositions have provided a basis for considerable "applied psychology," empirical research has sometimes raised doubts concerning their validity. In order to discover some of the limiting conditions under which the propositions are likely to be valid, it has been necessary to examine the conception of collective action on which they are based.

This examination has led to the fol-

lowing conclusions. Accurate social perception should promote interpersonal competence and group efficiency if: (a) the group members are motivated to cooperate; (b) the accurately perceived qualities are relevant to the activities of the group; (c) members are free to alter their own behaviors in response to their perceptions of other members; and (d) the behavioral changes which are a consequence of accurate social perception are the kinds which produce a more thoroughly integrated dyadic system. Whenever any one or more of these conditions is not met, accurate social perception should fail to have the effect predicted by the two propositions which have been the major concern of this paper.

It has been contended that much of our most highly integrated and efficient collective action must be presumed to occur within the framework provided by role systems. Whenever this is the case, individual participants are neither required nor permitted to let their perceptions of other people's intentions or preferences affect their behavior. Accuracy of social perception is largely irrelevant in such situations because it can have little effect upon individuals' behaviors. Indeed, if it does affect individuals' behaviors, it is likely to interfere with role enactment, and, hence, to disrupt the behavior synthesis which is provided by the role system.

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[2] The writer is at present conducting a laboratory study designed to test this contention.

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THINKING: FROM A BEHAVIORISTIC POINT OF VIEW [1]

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Hull (10) has demonstrated that the habit family hierarchy and related principles may generate many hypotheses concerning behavior in relation to objects in space such as might occur in the *Umweg* problem or simple kinds of novel behavior. He has thus shown that the elementary laws of behavior may be applicable to behavior of non-speaking organisms in so-called reasoning situations.

The purpose of this paper is to demonstrate that the principles formulated by Hull and by Spence may also be applicable to the problem solving of articulate humans. In this respect the present analysis has much in common with the important formulations concerning mediated generalization and problem solving by Cofer and his associates (2, 3), Dollard and Miller (5), Doob (6), and Osgood (19). The behavior theory involved may be outlined as follows. Behavior is a function of effective reaction potential ($_s\bar{E}_R$), which in turn is a multiplicative function of habit strength ($_sH_R$) and the effective drive state (D) minus the total inhibitory potential ($\bar{I}_R$). The latter represents the summation of reactive (I_r) and conditioned ($_sI_R$) inhibition. It is assumed here that the effective drive state represents the summation of the anticipatory goal response (*rg-sg*) as well as the primary and secondary drives (23). Furthermore, the multiplicative effect of the anticipatory goal response is restricted to its associated class of instrumental responses.

The principal theoretical conception necessary for our account of problem solving is an extension of Hull's spatial habit family hierarchy (9, 10). The great complexity of human thinking requires the formulation of what might be called compound temporal habit family hierarchies. In the spatial habit family hierarchy, alternative locomotor responses are elicited as a function, in part, of spatial and temporal distance from a goal. But in adult human problem solving, responses in changing spatial relations to a goal are not usually elicited, although there are problems involving motor skills in which this may be the case. A typical performance change in problem solving is in terms of verbal responses, and the change is solely a temporal one (4). Nevertheless, it is assumed that the principles operating in the spatial habit family hierarchy will to a large extent operate in the temporal hierarchy. Recent evidence in support of this assumption has been obtained by Rigby (21).

The conception of a compound temporal habit family hierarchy is based upon the prior assumption that the elementary laws of behavior derived from conditioning and applicable to trial-and-error and discrimination learning are also applicable, at least in part, to primary problem solving or reasoning, and thinking in general. That different kinds of behavior are observed in conditioning, trial-and-error, discrimination, and problem-solving situations is not to be denied. But these different behaviors need not necessarily involve fundamentally different laws. Different behavior is observed in these situations

because the initial conditions are different, and the situations represent varying degrees of complexity in the sense of the number of different variables and principles operating in them. Nevertheless, it is reasonable to assume that at least some of the elementary laws derived from conditioning will lead to the development of the composition laws operating in human problem solving.

As Hull (10) has demonstrated, these elementary laws can account for many of the phenomena of simple trial-and-error learning. A hierarchy of responses elicitable by a given stimulus, in which the correct response is relatively low in the hierarchy, characterizes this form of behavior, as shown in Fig. 1. Learning is said to be complete when the order of the response hierarchy has so changed that the correct response is now dominant in the hierarchy. Hull has called this hierarchy of responses, elicitable by a given class of stimuli, the divergent mechanism (9).

As Spence (22) and Hull (10) have demonstrated, the elementary laws of behavior derivable from conditioning situations can also account for many of the phenomena of simple discrimination learning. A hierarchy of stimuli eliciting a given response in which the
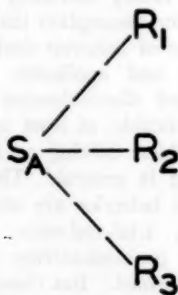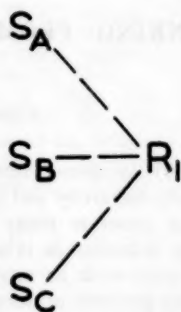

FIG. 2. A convergent mechanism. The alternative stimuli have varying tendencies to elicit a given response.

correct cue is relatively low in the hierarchy characterizes this form of behavior, as shown in Fig. 2. Such learning is said to be complete when the order of the stimulus hierarchy has so changed that the correct cue is dominant. Hull has called this hierarchy of stimuli eliciting a given response the convergent mechanism (9).

A synthesis of the divergent and convergent mechanisms gives rise to the habit family hierarchy involved in behavior sequences in relation to objects in space (9, 10). A hierarchy of this sort is shown in Fig. 3.[2]

As seen in this figure, S_A (the external stimulus) and S_D (an internal drive stimulus) are capable of eliciting a given habit family hierarchy and equivalent responses leading to a given

[2] There have been a few minor deviations from Hull in the manner of diagramming the stimulus-response relationships in order to simplify their presentation. Instead of having a dashed line between the stimulus and each response member of the divergent mechanism, a single dashed line leading to a bracket is used. All bracketed responses are associated with the stimulus. The number and length of the response sequences have also been reduced. A dashed line between a stimulus and a response signifies a learned association, while a solid line between a response and the cue it produces indicates an unlearned association.

FIG. 1. A divergent mechanism. The stimulus has varying tendencies to elicit the alternative responses.
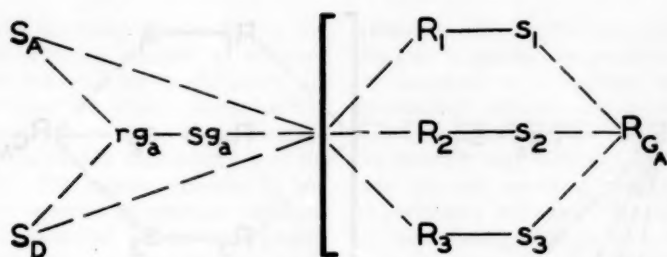
FIG. 3. A habit family hierarchy produced by a divergent and a convergent mechanism.

goal. Common to all responses in the hierarchy is a fractional anticipatory goal response (rg_a-sg_a) which is associated with both the external and internal sources of stimulation. Responses in the hierarchy may be elicited directly by either the external or internal stimuli, or by both. The effects of reinforcement or extinction of individual members of the hierarchy generalize to other members through the mediating mechanism of the anticipatory goal response and its stimulus. It therefore follows that the principles of conditioning and trial-and-error learning should also apply, at least in a general way, to the habit family hierarchy.

For example, if the reaction potential of the correct response leading to a goal is low in the hierarchy, then the generalized conditioned inhibition from the repeated failures of the dominant incorrect responses may reduce its effective reaction potential below the response threshold. Attainment of the goal would never occur under the conditions present. Or, if the subject has a high degree of an irrelevant need such as anxiety, aside from the possible interfering effects coming from the competing responses aroused by this drive, failure to attain the goal under the above conditions would be even more pronounced. The increased effective drive state multiplying the habit strengths for the dominant incorrect responses and the weak correct responses would increase the absolute difference in reaction potential between the two.[3] Such a condition would produce a greater amount of conditioned inhibition generated through the repeated extinction of incorrect responses. This in turn would increase the probability of failure.

Another way in which changes in the habit family may occur is through the arousal of the fractional anticipatory goal response (rg_a-sg_a). Its arousal may produce an immediate increase in effective reaction potential for the related responses. This effect occurs because it presumably enters into a multiplicative relationship with habit strength in the determination of reaction potential.

A synthesis of habit family hierarchies gives rise to the compound habit family hierarchy involved in human problem solving. A hierarchy of this sort is shown in Fig. 4. It is formed when the stimulus of a divergent mechanism becomes a member of

[3] For illustrative purposes we may substitute numerical values in the formula for reaction potential ($_sE_R = {_s}H \times D$). The $_sH_R$ value for the dominant incorrect response is 5; the $_sH_R$ value for the weaker correct response is 2; drive has a value of 1. The absolute difference in reaction potential between the correct and incorrect responses is therefore 3. If the drive state is increased to a value of 2, then the absolute difference between responses becomes 6. A greater difference in reaction potential must now be overcome before the correct response can become dominant in the response hierarchy.
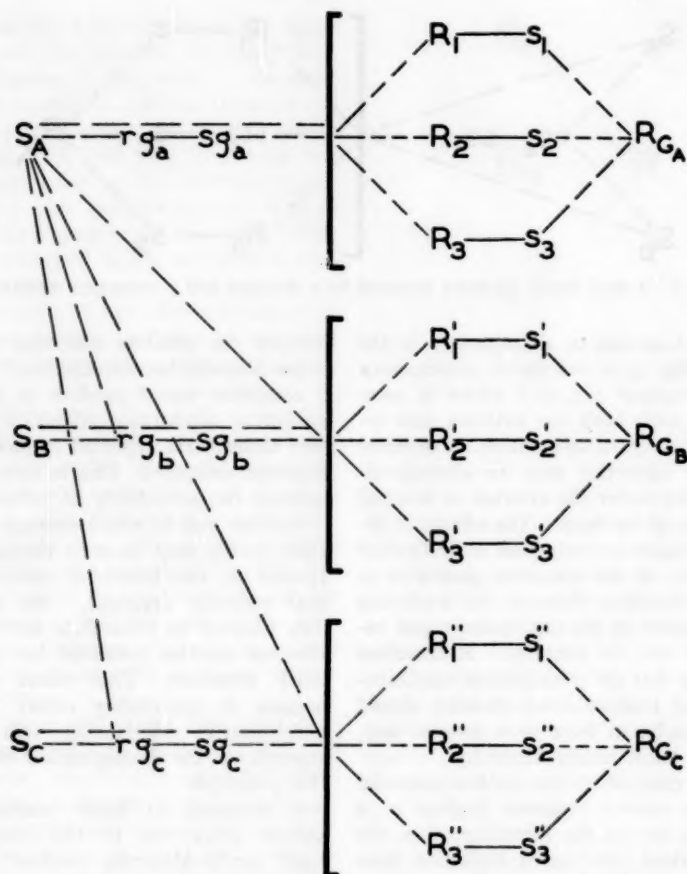
FIG. 4. A compound habit family hierarchy produced by a combination of habit family hierarchies.

a convergent mechanism as well. By the same learning process, responses of the divergent mechanism in question become responses in convergent mechanisms. In the compound habit family hierarchy not only does S_A have the disposition for arousing its habit family hierarchy, but to varying degree the habit family hierarchies of S_B and S_C as well. There is a hierarchy of habit families elicitable by S_A. An analogous condition holds for the other stimulus complexes, S_B and S_C. They have varying amounts of reaction potential for the elicitation of the other habit family hierarchies. These relations as well as the drive stimuli have been omitted in Fig. 4, in order to avoid confusing details.

When the compound hierarchy is formed, R_1, for example, originally only a member of the divergent mechanism elicitable by S_A, becomes a member of a convergent mechanism. There

is now a hierarchy of stimuli, S_A, S_B, S_C with differing amounts of effective reaction potential for its elicitation. A similar state of affairs exists for the other response members of a hierarchy, as well as for the anticipatory goal responses. The stimulus complex S_A has varying amounts of reaction potential for the elicitation of the anticipatory goal response rg_b-sg_b and rg_c-sg_c as well as its original anticipatory response. We now have a hierarchy of habit family hierarchies or a class of classes of stimulus-response relationships.[4] Thinking in general, and problem solving in particular, thus may involve the selection of habit family hierarchies as well as the selection of specific response sequences within a hierarchy.

If the selection of response classes or habit family hierarchies obeys the same principles as the selection of individual responses, then the task of discovering the principles of problem solving may be greatly facilitated. The laws derived from conditioning could then be used to account for changes in the compound hierarchy, without, of course, necessarily excluding other principles. Maltzman and his associates have obtained some experimental evidence in support of this assumption (16, 17, 18).

An additional basis for such an assumption is that the difference between instrumental conditioning and problem

[4] This presentation of the compound habit family hierarchy is an oversimplification. For one thing, it does not indicate that individual response members of a habit family may potentially serve as the anticipatory goal response for other habit families. Also, the stimulus aspects of the compound hierarchy, and how they are related to concept formation and perception, are not developed here.
Theories of problem solving by Duncker (7), Duncker and Krechevsky (8) and by Wolters (25), although stemming from different points of view, have a number of characteristics in common with the compound habit family hierarchy.

solving in this respect may not be as great as it seems. In problem solving the members of a response class are qualitatively different—for example, different verbal responses. Effects upon one member may influence other members through mediated generalization, as previously indicated. In instrumental conditioning, changes in a response class, however, are also involved, because precisely the same response may not occur on successive trials (10). There are differences in intensity and, perhaps, in quality in the successive bar-pressing responses in the Skinner box. Nevertheless the rate of bar pressing increases with successive reinforcements. Learning occurs even though a given response is not precisely repeated, because a class of similar responses is reinforced as a result of simple stimulus and simple response generalization. The limits and the precise manner in which generalization occurs in the two situations are probably different. But because of its important theoretical implications for a theory of problem solving, the similarity between the two situations in this respect should not be discounted.

Changes in the order of dominance in the compound hierarchy may occur as a result of either or both of two effects. First, the effective reaction potential of the incorrect dominant habit families and their individual members may be decreased as the result of extinction. The initial response elicited in a given problem situation would tend to be the dominant response in a dominant habit hierarchy. If this response does not lead to a solution, as by definition it would not, it would receive an increment in inhibitory potential, and responses next in the order of dominance would tend to occur. There would be temporary extinction and spontaneous recovery of these incorrect responses. Eventually the inhibitory potential of

these responses would reduce their effective reaction potential below that of the responses contained in the hierarchy next in the order of dominance, and so on. Each response in the hierarchy need not be elicited and extinguished, however, since mediated conditioned inhibition may generalize from one member of a hierarchy to another. The anticipatory goal response of this hierarchy presumably would also acquire inhibitory potential, thereby further reducing the effective reaction potential of the related class of responses.

A second general way in which changes in the order of dominance in a compound hierarchy may occur is by increasing the effective reaction potential of the habit family or families initially low in the hierarchy which contain the correct responses. One way in which this may come about is as a result of previous reinforcement of individual members of the hierarchy in that situation. Through mediated generalization all members of the hierarchy would receive an increment in reaction potential.

Another way in which a habit family may be raised in the compound hierarchy is through the arousal of the anticipatory response of the habit family. Elicitation of the anticipatory goal response produces an immediate increase in effective reaction potential for the related class of responses, for the reason previously mentioned (15). The antecedent condition for the anticipatory goal response is assumed to be commerce with a goal or a substitute, often symbolic, for the goal. In adult human problem solving the latter is the typical condition. Recent research indicates that verbal instructions given by the experimenter provide an important condition determining the arousal of the anticipatory response (2, 15). The consequent condition of its arousal is an increased probability of occurrence

of responses instrumental in attaining the goal in question.

These different ways in which changes in the compound habit family hierarchy may occur are related to the different kinds of problem solving distinguished by certain writers. As Hull (10) has pointed out, any learning situation other than classical conditioning involves problem solving of some sort. The essential characteristics of a problem-solving situation are that an organism is motivated, and that attainment of some goal object satisfying that drive is dependent upon the organism's performing in a given manner. It is characteristic of problem solving that the appropriate response leading to goal attainment does not immediately occur. This is true to some extent in instrumental conditioning, and to a greater extent in trial-and-error learning and in what has traditionally been called problem solving.

Further distinctions between different kinds of problem solving have also been made. Maier (13, 14) has repeatedly distinguished between different functions responsible for problem solving, and has classified problem solving as either productive or reproductive. The latter kind of thinking according to Maier involves the application of previously acquired experiences which lead to a correct solution in a new situation. It is problem solving based on the transfer of training, or equivalent stimuli. Productive thinking on the other hand is the consequence of the integration of previously unrelated experiences. The integration is produced by a direction which is an "outside" force not itself a habit.

The present treatment of problem solving, as previously indicated, makes use of the concept of the fractional anticipatory goal response. Certain functional characteristics of the anticipatory goal response appear to make it

an analogue of Maier's concept of a new direction. However, among other things, we do not accept Maier's restriction that only productive thinking involves the combination of previously isolated habit segments, to use Hull's terminology. To some extent this is the distinguishing characteristic of all thinking. It is the feature that sets it off from simple retention, and trial-and-error learning. All forms of thinking involve mediated generalization, and hence compounding of previously isolated habit segments.

Nevertheless, there are differences between reproductive and productive thinking. Situations eliciting reproductive thinking often involve the presentation of a succession of problems. The solution of each of these requires the elicitation of different response members of the same habit family hierarchy. This habit family will then become dominant in the compound hierarchy, as the result of reinforcement of its response members and the extinction of responses belonging to different habit families. As previously indicated, the increase in reaction potential of the entire class of responses as the result of reinforcement of individual members would occur as a consequence of mediated generalization. For example, subjects given a series of anagrams whose solutions all belong to the same word category will have a greater frequency of success on subsequent anagrams of the same category than subjects without such prior experience (20). However, failure on individual problems may occur because a solution is still dependent upon selection of particular responses within this one habit family.

The occurrence of reproductive thinking in situations where only a single problem is presented is also the consequence of the factors outlined. As a result of past training the dominant habit family in the compound hierarchy contains the correct response. Solution of the given problem depends upon extinction of the initially dominant incorrect responses within this one hierarchy.

In productive thinking, on the other hand, a habit family initially low in the compound hierarchy must become dominant before a correct solution can be attained. This occurs as the result of the extinction of the dominant incorrect response hierarchies. Once the appropriate habit family is dominant a solution will occur, provided that the correct responses within that hierarchy in turn become dominant. The protocols of subjects in Duncker's radiation problem exemplify this mode of thinking (7).

If a subject incorrectly anticipates the goal or solution to the problem—e.g., "destroy the tumor by means of rays sent over a path as free as possible from healthy tissue"—the proposed solutions are alternative responses within the same dominant habit family, and are all attempts to achieve this end. Repeated failure of these proposals will produce extinction of the anticipatory goal response and its related instrumental responses. As a consequence, another habit family may become dominant. The subject will now anticipate a different kind of solution, such as "reduce the intensity of radiation." As a result, all potential response sequences in the subject's habit family leading to this goal are facilitated. If the correct solution (converging rays from different angles) is not the dominant response in this hierarchy, extinction of incorrect responses must occur before the solution will be attained.

In an actual protocol this orderly progression from one habit family to another is probably an infrequent occurrence. The more typical case would

be one in which the reaction potentials of two or more habit families overlap. This would probably be the case after the first few responses from the dominant hierarchy have been extinguished. Thus reproductive and productive thinking differ with respect to the kinds of changes which must occur in the compound hierarchy before a correct solution can be attained. In reproductive thinking the habit families containing the correct responses are dominant at the outset of the problem, as the result of training and generalization from other situations. Or they rapidly become dominant in the compound hierarchy through the reinforcement of individual response members of the habit family. In productive thinking the habit families containing the correct responses are initially low in the compound hierarchy. They become dominant following the extinction of the dominant habit families which lead to incorrect solutions.

Experiments on the effects of direction in the pendulum problem by Maier (12) and by Weaver and Madden (24) are a special case of productive thinking, in that instructions and demonstrations are employed to increase the reaction potential of the anticipatory goal response and individual response members of the habit families leading to a correct solution. The problem is to construct two pendulums which would make chalk marks on two different places on the floor. In Maier's experiment (12) one group of subjects was given only the statement of the problem. Other groups received the statement of the problem plus various additional instructions or demonstrations. One of these groups was given three different demonstrations of operations on the material which were necessary for solution of the problem. They were shown how to make a plumb line, how to combine poles by using a clamp,

and how to wedge poles against a surface. A third group was given these demonstrations and told that they must combine them for a solution of the problem. A fourth group was told that it would be advantageous if the pendulum could hang from the ceiling. They were given a "direction." The fifth group received the demonstrations and the direction. All of the problem solutions except one occurred in this last group.

According to the present formulation the experimenter's statement that it would be advantageous to hang the pendulum from the ceiling tended to elicit an anticipatory response for this goal. A wide variety of equivalent responses instrumental in leading to this goal therefore received an immediate increment in reaction potential. However, for a correct solution to occur, certain specific responses must be elicited. The three demonstrations given the last group increased the tendency of these responses to occur within their respective habit family hierarchies. The increased frequency of solutions under these conditions would follow from the differential increase in reaction potential of the relevant responses and the lawful nature of trial and error learning. However, since a large number of responses belong to a given hierarchy, and the correct responses may still not be dominant, extensive extinction of incorrect responses must occur. If the correct responses are very low in the dominant hierarchy, a solution may not occur at all because of the extensive generalization of the effects of extinction of the incorrect responses. Another basis for failure in this group is that despite the instructions tending to arouse the appropriate anticipatory goal response, some subjects, presumably as the result of self-instructions, induce different anticipatory goal responses. These are the subjects that adopt an

inappropriate approach to the problem, according to Maier.

Groups 2 and 3, which are not given the directional instructions but receive the demonstrations, also have an increase in reaction potential for the three response sequences necessary for a solution. However, the anticipatory goal response and the related class of responses necessary to suspend the pendulum from the ceiling are not increased in strength, which is presumably why these subjects failed to solve the problem. However, contrary to Maier, Weaver and Madden (24) found no difference between the performance of groups 3 and 5.

Their experiment implies that the appropriate habit family for suspending the pendulums from the ceiling may be elicited by self-instructions, or that the increase in habit strength of the three necessary response sequences by itself may be sufficient for the solution of the problem in some individuals. Why there was this discrepancy between the two studies, however, is not at all apparent.

Throughout the previous discussion the systematic and theoretical status of the concept of thinking has only been implied. We shall now try to make it more explicit. According to the present systematic position, thinking is a defined concept or hypothetical variable. The specific definition that is given to it is the problem of theory, and will be discussed presently. Now we must explore further the consequences of the assumed systematic status of the concept. For one thing, thinking is not a response, verbal or otherwise, just as learning is not a response, and just as electricity is not the temperature of a conductor. All of these are dispositional concepts that are given empirical meaning by statements referring to their antecedent as well as their consequent conditions (1).

They are not equivalent to their manifestations or consequent conditions. The insistence that thinking *is* a verbal response, a contraction of certain muscles, or activity in the central nervous system, is thus based on an inappropriate use of language. The verbal response, for example, is just one of several different kinds of responses that may be taken as a criterion or manifestation of thinking. Other response criteria might be gestures, mimicry, motor skills, etc. Questions as to which ones may be taken as criteria, and under which conditions, as well as how they are related to thinking, are to be answered by experiments and theory.

The present systematic position with respect to the relationship between thinking and verbal responses (the most frequently used criterion of thinking) may be made clearer by using the analogy of bar pressing in the Skinner box. The assumptions here are analogous to the assumption that bar pressing is a function of other variables besides learning. If a bar depression does not occur, it need not necessarily imply the absence of learning. The rat's motivation may have been reduced to a minimum; there may be temporary extinction of the response, or inhibition due to the arousal of competing responses by extraneous stimuli, etc. Similarly, the absence of a verbal or some other kind of response does not necessarily mean the absence of thinking. It may be due to the absence of effective reaction potential for that particular response; perhaps the relevant motivation is absent; or other response tendencies inhibit its appearance, etc. On the other hand, the presence of bar pressing does not necessarily imply that learning has occurred. It may be an operant level, some unconditioned response strength—in Hull's terminology, $_sU_R$. Likewise, verbal responses may occur in the absence of thinking.

One aspect of this condition will be discussed shortly.

In the foregoing we have tried to explicate the systematic status of the concept of thinking. We shall now turn to the more specific problem of how it may be treated within the framework of Hull's theory of behavior.

Since it is assumed that thinking as well as learning is a disposition or hypothetical variable, the problem now is to distinguish between the two concepts. If the two are defined in terms of the same operations and consequences, they have the same empirical meaning and the distinction is purely a verbal one. As commonly employed, the term learning refers to the acquisition of a hypothetical state, $_SH_R$, as a result of antecedent conditions such as the number of reinforcements. The consequent conditions are changes in some response criteria such as decreased latency, increased rate of responding, etc.

The term thinking as it is employed here refers to the utilization of new combinations of habit strength by articulate organisms. In other words, we assume that thinking is equivalent to a complex form of effective habit strength which is produced by mediated generalization. The reason for arbitrarily restricting the usage of "thinking" to humans is the belief that extensive mediated generalization is necessary for the recombination of habit strengths to occur, and complex mediated generalization of this sort is made possible primarily by linguistic responses.

We have stated that thinking involves the utilization of learning in new combinations, as distinguished from the acquisition of learning. A further problem is to distinguish between thinking and retention, since the latter also involves the utilization of habit strength. The distinction is not always easy to make, and at times may be arbitrary.

But so is the commonly accepted distinction between learning and retention (11). As previously noted, learning refers to the acquisition of a hypothetical state as a function of the number of reinforcements. Retention is a term referring to the persistence and subsequent manifestation of that hypothetical state. In a learning experiment, performance on trials after the first is a function of the persistence of previous learning, or retention, from earlier trials. Only on the first trial is nothing more than the acquisition of habit strength involved. This entire process, however, is called learning even though much of it is actually retention. After the subject has reached some predetermined criterion, he is required to utilize in some manner the habit strengths previously acquired. He is asked to recall the material previously learned, recognize, or relearn it, etc. In every case the responses originally acquired are elicited again to some extent. This implies that the habit strengths utilized in the test of retention are substantially the same as that originally acquired. Retention has as its consequent condition the elicitation of some previously acquired response, presumably as a result of the persistence of previously acquired habit strength. The term thinking has as its consequent condition the elicitation of a response other than the previously acquired response as a result of past learning. Habit strength previously acquired has entered into new compounds, has changed as the result of mediated generalization.

A fundamental problem for a behavioristic psychology of thinking is to determine the laws governing these combinations and recombinations of habit strengths. Hull's equations (10) for combining habit strengths in generalization and compound stimulus situations are approximations of such composition laws. But they are only

first approximations, because the generalization and compounding of habit strengths occurring in thinking are undoubtedly a good deal more complex than those that Hull has treated. A basic problem in this respect would be the development of the laws of mediated generalization which, theoretically, produce the new compounds of habit strength, and empirically, produce the formation of new stimulus-response classes. It is likely that the close connection between language and thinking (or even their equating) in certain theories results from the fact that language permits the greatest degree of mediated generalization and therefore thinking.

Admittedly the theory of thinking and problem solving outlined here is loosely formulated and incomplete. Nevertheless, it at least has the merit that it relates human problem solving to behavior in simpler situations. It is an attempt to integrate the two, as distinguished from the usual gestalt approach which treats problem solving as divorced from the relatively large number of principles derived from conditioning and trial-and-error learning. Although many of the principles derived from conditioning may not entirely apply to human problem solving, this is certainly an empirical question worth investigating. At the very least, these principles should yield significant hypotheses as to the factors determining problem solving.

SUMMARY

A theory of human problem solving has been outlined, based upon the concept of a compound temporal habit family hierarchy, which is assumed to function, at least in part, according to the principles of conditioning and trial-and-error learning. Some of the characteristics of the compound hierarchy were noted, and its role in different kinds of problem-solving situations was indicated.

The systematic status of thinking from a behavioristic point of view was described as a disposition or hypothetical state of the organism. Within the present theory it is equivalent to a new combination of habit strengths produced, primarily, by mediated generalization.

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VISUAL FIGURE DISCRIMINATION AND THE MEDIATION OF EQUIVALENCE RESPONSES

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Among the most prominent features of any group of discrimination performance data are (a) the extensive daily variations in performance level of any subject and (b) the differences among subjects in the total number of training trials required to attain a specified criterion performance level. Psychologists have devised several methods designed to minimize these characteristics and thereby to display a continuous function designated a "learning curve." The derived function is very often thought to be a "curve of learning." Such a manipulation seems implicitly to ignore, or even to deny, the tenet in psychology that all behavior is determined, and to recognize certain behaviors as "chance or random" much after the manner of Aristotle. The psychologist, as a behavioral scientist, ought not to slur over this inconsistency in behavior, but rather should deal with the problem in his thinking and his theorizing. One attempt to face up to the problem is tendered here without further apology.

In 1932, Krechevsky (2, 3), extending Lashley's notion of "attempted solutions" (4), endeavored to account for discrimination learning in terms of "hypotheses" during the presolution period. The basic supposition of the notion is that, so long as no hypothesis involves the differential cue stimuli, associations are not formed to these cue stimuli, and hence the discrimination problem is not solved. Errors following solution of the problem are irrelevant to the formation of the association, and are not adequately accounted for in this system. Spence, on the other hand, believed that discrimination learning could be handled in terms of trial and error learning theory, and in 1936 (7) published a theoretical formulation based upon the principles of reinforcement and nonreinforcement of discriminably different stimuli, making learning a continuous process. Hypothetical generalization curves were presented (9) which showed performance at any time during training as a function of the interplay of excitatory and inhibitory tendencies with respect to the cue stimuli. It was made explicit that weakening, or inhibition, follows nonreinforcement only when the stimulus already has a certain minimum excitatory strength (8). The notion does not adequately explain the magnitude of the variability seen in discrimination performance data, for it predicts relatively uniform increments in performance level during successive training periods.

TWO ASSUMPTIONS

Because of the writer's bias toward a continuity theory, an attempt will be made to extend this type of formulation to account for both the wide variability in performance from one training session to the next and the differences in rate of acquisition, and also to account for the character and extent of transfer of an acquired visual figure discrimination to variations from the training figures. The idea was developed during a discrimination study with pigeons (10), but tentatively will be treated as though descriptive of mammalian behavior as well. Two assumptions are necessary to the formulation,

one seeming axiomatic and the other being less obvious and having no direct experimental support. The first assumption is that any visual figure may be perceived in several different ways, i.e., a visual figure has several aspects to which an organism may learn to react. Secondly, it is assumed that the organism does not perceive the same visual figure in the same way on all trials or training sessions, i.e., the organism does not always react to the same aspect of the stimulus from one trial or training session to the next.

The term "aspect," as used above, requires some discussion. Aspect will *connote* a unitary and organized perception involving a portion or property of a visual figure as "figure." Aspect will *denote* that portion or property of a visual figure which is necessary and sufficient for a differential conditioned response by a particular organism, when confronted with that portion or property alone or in conjunction with one or more additional visual figures or portions or properties of them. Thus if, after a positive response to a triangle and avoidance of a square were established, a line of the same length and orientation as the base of the triangle evoked a positive response when appearing alone or with the square, or any other figure, and would not evoke a positive response when its length or orientation was altered, then that line in that orientation would be an aspect. Similarly, any other part of the triangle, e.g., an angularity property, which evoked a positive response when presented alone would be an aspect. Aspects are additive, divisible, or reducible for any organism; they need not be absolute properties, but may also be relative properties among figures. It must be pointed out that these aspects, in contrast with Hullian elements, are not necessarily the units which a physicist may find, but are momentarily unitary with respect to the perceiving and reacting organism. In short, there are certain absolute properties in any visual figure and certain relative properties among visual figures to which an animal could learn to respond, which will then be called aspects or stimulus aspects. Hebb (1, p. 105) has developed a notion very much the same as this in his schema.

If an animal perceives a portion or property of both the positive and the negative figures in the same way, an ambiguous aspect emerges. Those portions or properties of the figures giving rise to the same perceptual response may or may not be physically identical. If they are not identical in the physical sense, the animal may at some future time differentiate them (5, 6). When differentiation occurs, the "new" aspect of the negative figure may now acquire an inhibitory tendency. That is, when an excitatory value is attached to an ambiguous aspect which is subsequently perceived as two different aspects, an inhibitory tendency may be built up to the new aspect of the negative figure (after Spence), and performance will then show improvement of a step-wise nature.

SOME PREDICTIONS

Because the mere germ of the idea has been sketched here, a few predictions deriving from it might elucidate the theory. Several relations follow from the above formulation.

1. As the criterion performance level for initial learning is raised, the amount of transfer to new figures will increase. The variety of visual figures to which an animal responds, as equivalent to the original positive training figure, depends upon the number of positive and negative aspects that the animal has acquired during training. The longer the subject is exposed to the training figures, the greater will be the number of aspects acquired. It follows that

transfer will be more extensive with greater amounts of training.

2. As training progresses, the ratio of adient to avoidance responses will change. In the initial stages of learning, if no transfer from prior experience is involved, all responses should be adient in nature. As training progresses and ambiguous aspects are split into positive and negative aspects, there should be an increasing number of responses directed largely by avoidance of certain aspects. This visual avoidance of negative aspects will increase the probability that the animal will respond to a positive aspect of the other figure; it is believed that the negative stimulus aspect does not itself result in the conditioned behavior of responding to the "other" figure. The work of Lawrence (6) also hints at this sort of mechanism. The portion or property of the figure which has no associated inhibitory value will not direct the visual exploration of the animal away from the "negative" figure, and thus should play no role in transfer behavior.

3. The amount of training required to attain a specified criterion will vary with the complexity of the figures employed. Very simple figures have relatively few potential aspects, i.e., will be perceived in a limited number of ways. When few aspects are involved, performance should improve quite rapidly. If the rate of "shopping about" is independent of figural complexity, then the total number of trials required to attain a certain performance level will increase with increases in the number of ways the animal perceives parts of the figure.

4. The ratio of adient to avoidance responses will vary with the complexity of the figures used. Avoidance responses will be least for simple and grossly different figures, and will increase as the complexity and similarity of the figures increases (increasing the probability that ambiguous aspects will

develop). Changes in the ratio of ambiguous to positive and negative aspects have significance for the problem of experimental neurosis.

This kind of formulation can be extended outside the realm of visual figures to account for the form of the generalization gradient and such phenomena, but will not be attempted here. It should be pointed out that different species will probably display various degrees of the tendency to "go shopping"; the extent of transfer to altered figures will be related to this tendency. Whether or not a particular figure is responded to positively depends upon how that figure or a part of it is perceived at the moment.

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ELICITATION THEORY: I. AN ANALYSIS OF TWO TYPICAL LEARNING SITUATIONS

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The present paper will be concerned with an acquisition hypothesis and one or two other elicitation principles which constitute part of a general theory of behavior now under development.[1] However, there will be no attempt to present the theory here in a formal or definitive manner. Our purpose is to indicate the operation of the theory by applying it in the analysis of two types of instrumental learning, namely, in a conventional **T** maze and in a Skinner box. Simple approach learning in a runway will not be discussed because its explanation can be interpolated from the analysis of the more complex situations.

THE ACQUISITION HYPOTHESIS

The acquisition hypothesis, which has its roots in both Guthrie's contiguity principle and Hull's reinforcement principle, may be stated as follows: (*a*) The stimulus complex (*S*) which closely precedes in time any response *elicited* by any stimulus (S_e) acquires the property to elicit this response. (*b*) With each elicitation there results an increment to the tendency of the stimulus complex (*S*) to elicit this response. (*c*) The *S-R* association will not ordinarily be evident in behavior unless the response is *consistently* elicited in the given stimulus situation ($S + S_e$). Thus, for all practical purposes, learning occurs only when a response is prepotent over

a series of trials or over an extended period of time.

Two terms require explication. By *elicitation* is meant that the response is evoked in some manner which necessarily involves the afferent nervous system. We use *consistently* to refer to whether or not the response is elicited each time the stimulus (*S*) is present: The more often *R* is elicited each time *S* is present, the more consistent the elicitation.

In other words we are postulating that the essential condition for the strengthening of one response tendency over and above another response tendency is the *consistent* elicitation of the response in question. For example, the *US* of a classical conditioning experiment typifies a consistent elicitor. Here, it is appropriate to point out that conditioning of a specified response will not occur until all irrelevant elicitors have been eliminated. For example, Pavlov's dogs could not be conditioned until they were habituated to the total experimental situation, that is, not until the "neutral" stimuli in the conditioning situation had lost their eliciting property through a process of stimulus satiation or adaptation (7, 9).[2]

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[1] The present theory in many respects is parallel to a theory under development by Maatsch (**14**) in the same laboratory. It has similarities to ideas currently being expressed by Nissen (**16**, **17**), and by Sheffield and others (**20**, **21**).

[2] The principle of sensory elicitation and adaptation is fundamental in the present system. This principle refers to the fact that though all stimuli have some initial eliciting property most of them soon lose or partially lose this property through continued or repeated presentation. The stimuli most resistant to adaptation are the common reinforcing agents (food, water, shock, etc.). So-called neutral stimuli constitute the class of stimuli which commonly evoke investigatory, exploratory, and manipulatory responses (**7**, **9**, **15**). Such responses fall into the more general cate-

Another example of a consistent elicitor as found in an instrumental learning situation is familiar food for a hungry animal or water for a thirsty animal. In this case the approach to the goal object and the consummatory response are the responses consistently elicited. Since in a maze these responses are usually the only ones consistently elicited, they are the ones which are conditioned to the cues of the goal area.

Noxious stimulation is also a consistent elicitor and produces learning of the responses elicited to the contiguous stimuli, e.g., escape, fear, and avoidance.

Derived or Secondary Elicitation

There remains at least one more important class of eliciting agents. Once a behavior sequence has been established to some degree and the response no longer leads to the goal object or is no longer followed by the *US*, then there exists a condition for the elicitation of a new class of characteristic responses. This is to say, for example, that nonreward or frustration, either terminal or momentary, constitutes an eliciting (reinforcing) state of affairs.[3] In experiments involving nonreward it is customary to observe the characteristic response of recoil after entering a

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cul or negative end box, and of attempting to escape the place where reward was previously obtained (5). Since the omission of a goal object consistently elicits avoidant-type responses, and since the elicitation of these characteristic responses constitutes an instance of reinforcement,[4] the cues associated with nonreward acquire the property to elicit avoidance. With continued strengthening these avoidant responses compete with the original response and extinction of the approach response results.

We also postulate that the omission of noxious stimuli will elicit responses antagonistic to the original response. In general the responses which arise through the withdrawal of noxious stimuli belong to a class of "relaxational" or approach responses. Consistently elicited relaxational-approach-type responses are therefore conditioned to stimuli associated with the withdrawal of a noxious stimulus, and with continued strengthening compete with the original response and produce extinction. Experimental evidence in support of this notion may be found in two recent studies (4, 22). In a subsequent paper we hope to show that the eliciting property, rather than the "drive reducing" property, of the cessation or omission of shock is the *sine qua non* of typical instrumental-avoidance learning.

In summary, secondary elicitation, i.e., the eliciting of response by the omission of the *US* in a previously established behavior sequence, constitutes an instance of reinforcement just as the elicitation of any other response by an

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gory of approach behavior. With the passage of time all stimuli recover their capacity to elicit responses, approach or otherwise. One result of this phenomenon is variability in behavior, e.g., spontaneous alternation.

[3] In this connection it should be noted that Amsel and Ward (3) have demonstrated the cue properties of frustration, and Amsel and Roussel (2) have demonstrated the eliciting (drive) properties of frustration. In addition, Goer (8) has shown that as few as 12 trials reinforced on the same side as the rat's original position habit will facilitate the learning of a subsequent black-white discrimination. Presumably, the shift from consistent reward of the position response to 50 per cent reward sets up secondary elicitation and mediates the learning of avoidance reaction to positional cues.

[4] Reinforcement in the present context does *not* refer to the strengthening of a closely *preceding* response in the Hullian or Thorndikian sense. Reinforcement occurs simultaneously with the eliciting of the response, that is, it *is* the eliciting of a response. In similar fashion, a secondary reinforcing stimulus is simply a discriminative stimulus or conditioned elicitor.

US constitutes an instance of reinforcement.[5]

APPLICATION

Since the present position posits that all learning is fundamentally classical conditioning,[6] the task is to explain all

[5] This means that the laws of acquisition apply to extinction as well as to original learning. The only difference is with respect to the eliciting agent and how it functions. Thus it is quite possible that certain conditions which favor original learning may not at all favor the learning which takes place during extinction, and vice versa (1). The net result is to conceive of extinction as an interference phenomenon. The response tendency established during extinction (on the basis of secondary elicitation) competes with the response tendency of acquisition and eventually predominates, at least temporarily.

Discrimination is also involved. Any alteration of the stimulus conditions when shifting from acquisition to extinction, as in terms of spaced vs. massed trials, will facilitate the extinction process. If, as is typical, acquisition is spaced and extinction is massed, the introduction of a rest interval will tend to reinstate the "spacing" cues of acquisition and remove the "massing" cues of extinction. The net result is "spontaneous recovery."

In this interference theory of extinction there is no gradual unhooking of responses, as in Guthrie, and there is no weakening of the original response tendency by "non-reinforcement." It is simply that for *similar* stimulus situations there are two or more competing response tendencies.

[6] It should be made clear that in considering all learning to be a matter of classical conditioning we are not drawing a strict parallel between conventional *CR* experiments and simple approach or avoidance *CR*'s established during instrumental learning. The conditioning of an eyelid *CR*, for example, involves considerable discrimination learning in terms of specifying the *CR* to the momentary *CS* and *not* to the stimulus complex in general. Since typical *CR* experiments necessarily involve discrimination (18), *S* must learn to make some response other than the *CR* when the *US* is omitted between trials. If secondary elicitation is minimal or weak, as would be the case during the omission of a puff of air to the cornea, or a blow on the patellar tendon, or a bright light to the pupil, then many typical *CR*'s should be difficult to set up. Furthermore, in many

instrumental learning within this framework. To this end we will analyze two typical instrumental learning situations.

Learning the T Maze

Let us assume that we are attempting to train a group of rats to turn (right) into arm A of a conventional T maze where the extra-maze stimuli are discriminative. All *S*s have been placed on a reduced food regimen and handled for one week; no pretraining has been given; and a small pellet of food will serve as a reward.

Correction Technique

S is placed in the starting box. The novel stimuli of the T maze then elicit exploratory (approach) response, and *S* will eventually enter the positive end box at the end of arm A where approach and eating responses will be elicited and thus conditioned to the cues of this box. Somewhere near this point, stimulus generalization becomes an important factor. Thus the to-be-negative arm (arm B) which is similar to arm A will also acquire the tendency to elicit approach response. This, taken in conjunction with the fact that the less recently visited stimuli of arm B will possess greater exploratory value (approach elicitation), increases the probability of *S*'s entering arm B on some succeeding trial.

When *S* does enter the negative end box (arm B), the *US* (food) is not present. Therefore, secondary elicitation obtains, and the conditioning of avoidant

typical *CR* situations, particularly for those using human *S*s with uncontrolled attitudes, the *US* may not be the prepotent stimulus on every trial, and as a result conditioning never reaches a high level (12, 19). However, when a dominant elicitor such as food for a hungry animal is employed, both the presence and omission of the goal object are prepotent elicitors on every trial. Therefore, the conditioning posited to account for instrumental learning is rapid and rises to a high level.

behavior is initiated. Since in the correction technique recoil or escape responses can clearly occur after each wrong response, these responses will become conditioned to the cues of arm B, and eventually these cues will become signals of avoidance rather than approach. With continued trials (reinforcement) the cues of the choice-point region consistently elicit avoidance to B and approach to A. Thus, T-maze learning within this framework is regarded as a case of discrimination (perceptual) learning in which the stimuli of the choice-point region acquire the discriminative property to elicit avoidance responses to one arm and approach responses to the other.

In the context of the present theory it is necessary to point out that the responses (exploratory) elicited by novel stimuli will not be conditioned to the maze cues in a differential manner.[7] These responses are not consistently elicited by one particular side of the maze. Rather, on the basis of exploratory considerations alone, one would expect spontaneous alternation (6, 7, 15). However, the avoidance tendency conditioned to arm B will compete with the exploratory approach tendency and will virtually preclude entrance into that arm after a large number of trials.

Noncorrection Method

The explanation of learning a T maze with the noncorrection method is essentially the same as with the correction method. However, the fact that S is not allowed to recoil from the negative end box and also enter the positive end box on the same trial in the noncorrection method makes it necessary to treat these two training methods separately. Presumably, detention in the negative end box decreases the probability that

[7] Some learning of a nondifferential sort presumably does occur, and constitutes what is known as "cue familiarity" (11, 13).

any one response will be consistently elicited or specified to the cues of the negative end box. Also the cues of the choice point area in arm B are not directly associated with escape responses, i.e., there is greater S-R asynchrony than with the correction method. It therefore follows that the learning of avoidance to the cues of arm B will be appreciably slower than with the correction method and that learning of the correct response with the noncorrection method accordingly will be slower (10). Avoidance learning to the negative side, although less than with the correction method, nevertheless does occur. It has been found, for example, that the learning of a T maze (noncorrection method) is enhanced when the number of rewards on the correct side is held constant (5).

Place vs. Response Learning

From the point of view of the present theory there is no real controversy as to whether S learns a right-turn response or learns to go to the correct place (arm A). The concept of learning a turn response can only mean that the cues directing the response remain constant regardless of the orientation of the stem with respect to the arms. And in any T-maze situation a number of tactual, kinesthetic, and visual patterning cues in the choice-point area remain constant and thus mediate response learning when the cues beyond the choice point are nondiscriminative. Once S has made a response at the choice point under pure response learning conditions, there are no longer any stimuli in the maze which are discriminative other than a perseverative stimulus trace. For the cues of each arm, including extra-maze stimuli, will have acquired identical approach (avoidance) value. In Hullian terms this means that pure response learning involves delay of reinforcement; in the present con-

text we would prefer to say that pure response learning involves a long *CS-US* interval, i.e., a lapse of time between the cessation of the discriminative stimulus and approach-to-food. As previously implied, any deviation from strict contiguity of *S* and *R* will retard the establishment of the *S-R* tendency. On the other hand, in place learning, the stimuli of each arm are discriminative in a continuous fashion from the end box to the choice point area, i.e., there is an overlap of *CS* and *US*, and therefore faster learning (23) than under pure response conditions.

Learning in the Skinner Box

Let us assume that we are attempting to train a rat to depress a bar in a conventional free-responding Skinner-box situation. A small pellet of food will be used as a reward and the food tray is located in the vicinity of the bar. *S* is given a minimal amount of food association to the food tray. Then the bar is introduced and thereafter food is delivered only following a full bar depression. Release of food is immediate and is accompanied by a "click."

The initial behavior of *S* in the box consists of exploratory behavior including operant bar depressions. However, the first definite response which appears to be learned is approach to the food tray. Precise quantitative data on the early learning of this approach response have been obtained by Hurwitz.[8] Initially then, as would be clearly predicted from our acquisition hypothesis, *S* simply learns to approach the food tray—both during the food association period and in the early stages of free responding to the bar. After this approach response has been established, *S* will approach the food tray without first having pressed the bar. Since on this occasion there will be no food in the

[8] Personal communication from H. M. B. Hurwitz, Birkbeck College, London.

tray with which food association has been established, the conditions for secondary elicitation exist. Consequently, all cues associated with *direct* approach to the food tray will acquire the property to elicit avoidant-type response. The responses elicited by the empty food tray include retreat from the tray and an increase in the number and vigor of tangential responses. With respect to bar pressing, this frequently results in a "flurry effect" or a high bar-press/approach-to-food-tray ratio. After one or more bar presses, *S* will eventually reapproach the food tray and procure the food pellet, for any avoidance to the tray has only been incipiently established. A variable which can facilitate the learning of approach to the food tray subsequent to pressing the bar is the response-produced click which has been previously associated with the presentation of food.

It is now appropriate to point out that *S* in a Skinner box is confronted with a discrimination situation. In part, this is to say that certain cues are associated with approach to food tray, and certain cues are associated with avoidance of the food tray. In addition, since it is likely that a flurry of bar presses has been elicited (reinforced) through secondary elicitation, it is necessary to explain why *S* learns to press the bar only once per visit to the food tray. The principal discriminative stimuli employed by *S* in learning to approach and press the bar *prior* to visiting the food tray are presented below in order of establishment: (*a*) Proprioceptive, visual, tactual, and auditory elements which occur immediately after pressing the bar; (*b*) those stimuli involved in addressing the bar; (*c*) those stimuli associated with orienting toward the bar.

The manner in which these cues mediate the chaining of the response elements of the bar-pressing sequence

will now be discussed. To start with, "gross" or unconditioned manipulation of the bar results in unique stimulation which frequently involves a distinctive "click." These stimulus elements or their perseverative trace eventually occur contiguously with the response of approaching the food tray. Such stimuli therefore acquire the property to elicit approach to the food tray as soon as they occur. This means that S will learn to approach the food tray as soon as he presses the bar, i.e., when S hears the "click" or is otherwise stimulated by this distinctive stimulus complex. This also means that partial responses which do not activate the food release mechanism do not produce the stimuli which elicit approach to food tray.

Given the establishment of this first major link in the behavior chain (approaching the food tray *after* pressing the bar), it becomes necessary for us to consider the chaining of the second link (pressing the bar *before* approach to the food tray) to the first link. This is straightforward. Since visual, tactual, and proprioceptive stimuli of the bar area occur in contiguity with bar pressing and approach to bar, they become conditioned to pressing and approaching the bar. Another factor which is essential to the chaining in of this response link is the fact that when S fails to press the bar or makes an incomplete bar-pressing response before visiting the food tray, avoidant-type responses to the food tray are elicited (reinforced). In short, cues associated with direct approach to the food tray and "short-cutting" responses acquire avoidance properties; thus these responses are extinguished. Therefore, through the interplay of primary and secondary elicitation the instrumental learning of a bar-pressing response turns out to be an elimination of one behavior sequence and the introduction and establishment of a more complex sequence. Or, as in the case of T-maze learning, instrumental bar depressing resolves itself into a case of discrimination learning.

Evidence in support of this discrimination-type analysis is available in a study from our laboratory. In this experiment, after bar pressing had been established to asymptotic level under continuous reinforcement, groups of Ss were given varying amounts of 4:1 fixed-ratio reinforcement. A "click" accompanied only the to-be-rewarded bar press. Approach to the food tray following each of the first four bar presses in a fixed-ratio block tended to become extinguished. Following the fifth or to-be-rewarded bar press, S continued to visit the food tray. Presumably, the first four bar presses of a block tend to "discriminate out" and become chained in an instrumental fashion to the terminal or to-be-rewarded bar press. Given perfect discrimination, all responses of a block would be chained together and would constitute a single response unit. However, the end result of *any* amount of discrimination during the fixed-ratio schedule is to increase the number of individual bar presses which will occur during extinction. In the study being considered, it was found that resistance to extinction was *proportionate* to the level of discrimination attained.

SUMMARY

The present paper represents an attempt to explain two major types of instrumental learning within the framework of elicitation theory. Learning in a T maze was described as a case of discrimination learning, or a matter of learning to approach the cues on one side of the choice-point area and to avoid the cues of the other side. Pure response learning was contrasted with place learning.

Learning a bar-pressing response in a Skinner box situation was likewise described as a case of discrimination of a

somewhat more complex nature. Learning to make a discrete bar press followed by an immediate approach to the food tray is a matter of the elimination (extinction) of a behavior sequence of directly approaching the food tray, and the establishment of a more complex sequence of first approaching and pressing the bar followed by an approach to the food tray. Experimental evidence in support of this interpretation was cited.

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THE PSYCHOLOGY OF MENTAL CONTENT RECONSIDERED [1]

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Psychologists used to be interested in what went on in people's heads. In fact, for thousands of years this was practically all they were interested in. Psychologists from Aristotle to John Stuart Mill were concerned primarily with ideas and associations between ideas, but with the rise of modern scientific psychology we lost interest in ideas, by and large. The history of this development is well known, but let us review it for a moment. The psychology of mental content collapsed in the United States under the impact of two heavy blows. First, introspectionism seemed to run into a dead end. Titchener had argued manfully for a scientific study of the contents of mind, a kind of mental chemistry in which the basic elements would all be discovered and sorted out, but his laboratories simply failed to produce enough data to back up his theoretical position. It was not so much that his position was untenable. It was just that the data collected by the introspectionists did not seem to lead anywhere—to fruitful hypotheses, for example, which would serve to make theoretical sense out of the flux of mental events.

The second blow was even more devastating. It was, of course, the behavioristic revolution. Particularly in the United States, psychologists began to argue that conscious content could never form the basis of a science, whereas behavior could. J. B. Watson led the revolt in the name of scientific objectivity. After all, could you see or touch or feel or record with a machine a thought or a feeling? Now, a muscu-

lar contraction—an eye blink, a foot withdrawal, a right turn in a maze—that was something else again. That could be seen and touched and felt and often recorded entirely automatically by an impersonal, mechanical gadget. Here was the stuff of which a real science could be made!

- Looking back with the perspective of 30 years we can begin to see why this movement was so appealing. In the first place, it did provide the kind of objectivity, methodologically speaking, that psychology had never had before and it could, therefore, lend real support to psychology's claim that it was a science. Secondly, it fit in with the traditional American pragmatic bias in favor of action rather than thought or feeling which were generally considered to be old-fashioned European concepts. After all, in the United States it is what a man does that counts, not what he says or thinks or feels. This tendency in American psychology is still so prevalent that to many of us prediction of behavior means *only* predicting gross motor behavior rather than predicting thoughts, conflicts, doubts, imaginings, feelings, etc. as reflected in verbal behavior. Thirdly, behaviorism tended to focus attention on problems which were of vital topical interest to a new country in which many of its citizens were attempting to adjust to new ways of life. In fact, adjustment or *learning* became the key concept. And this was natural in a country in which so many immigrants or their children had to give up traditional European ways of behaving for new American habit patterns. It was at this point that psychology became almost exclusively interested in "process variables," in how people went

about doing things rather than in what they did. This was the time when Woodworth was stressing that we should rewrite psychology in terms of "ing" words—e.g., perceiving, emoting, thinking, learning, etc. No one was interested in *what* people thought, *what* they perceived, *what* they learned, etc. Instead we were to be concerned only with the laws which governed the *process* of perceiving, learning, etc.

Even personality and social psychology, which by definition are content oriented and which, therefore, should have resisted this trend, fell under the spell of this widespread movement. In personality psychology we were primarily interested in self-descriptive inventories in which the subject answered a lot of questions about his aches, pains, and anxieties. But, mind you, we did not look at his answers. We added them up to get a neuroticism score or dominance score or what not. We were not interested even here in *what* he said about himself, in *what* his ideas were. We were only interested in the extent to which his answer contributed to a total score which meant something else. To be sure, an individual clinician sometimes went so far as to look at the actual answers a person had given on a personality inventory, but then, he was not a scientist! In social psychology, too, we managed to get along without much concern with content, although here, too, it was a little difficult. The problem was solved with the help of the attitude concept. An attitude is essentially what I have been calling a process variable. We are interested in *how* attitudes get set up, in how we can measure them, in their consistency, in their rigidity, their generality or specificity, etc.—all process variables. But we are not interested in *what* they are, particularly. Any old attitude will do for our purpose, just as in studying learning, any old task will do for our purpose—a maze, a bar to press, a list of nonsense syllables, or what not. So social psychologists chose as the attitudes to be investigated whatever happened to be of current interest at the moment —e.g., internationalism, feminism, pacifism, race prejudice, and so on. Few if any people thought it was even worth asking which attitudes were the "important" ones to use in describing a person or his culture. Many people probably would have wondered whether such questions really fell within the province of psychology at all.

It is against the background of this widespread social movement in psychology that we can see the beginnings of the projective testing movement as the source of a change in attitude which is finally beginning to be felt today, possibly in large part because of the success of projective tests. But certainly projective testing did not start as a conscious revolt against this interest in process. Quite the contrary. The Rorschach test, as one of the oldest of these new instruments, probably gained as wide acceptance as it did in the United States largely because it became primarily process-oriented. It became concerned with *how* people perceive and only secondarily with *what* they perceive. Quantitative indices could be computed according to how many responses were determined primarily by form, by color, by movement, and the like. Nevertheless, the good clinicians often found that the particular content of the association given by the patient was of value to them in understanding the person. And this has always been the case with a good clinician. He *has* to be interested in *what* his patient thinks as well as in how he thinks it. Even though his formal psychological training gives him very little assistance at this level, he knows that in order to handle this particular person he has to be interested in the patient's ideas. This, it seems to me, has been the great and continuing contribution of the clinic

and the projective test in a time when psychological theorists have talked themselves out of being interested in content altogether. I am reminded here of a comment made to me once by one of my more cynical colleagues who claimed that no new ideas of importance ever appeared in the universities. Usually they appear outside first, and then are only gradually claimed by the universities. Certainly if we think for a minute of men like Descartes, Darwin, Freud, or Einstein, there would seem to be something in what he said. And this development seems to be a case in point. The projective testing movement grew up largely *outside* the conservative academic tradition and finally, because of its clinical success, has managed to dent the calm assurance with which many theoretical psychologists have discarded all problems of mental content.

But to continue with my story: The real change came with the development by Murray and his associates of the Thematic Apperception Test about 20 years ago. Now for the first time we had an instrument in which the primary concern was not form but content. The person interpreting a TAT record must ask such questions as: *What* motives activate this person? *What* conflicts plague him? *What* modes of defense does he adopt? *What* characteristics does the world have for him? No longer are we concerned primarily, as in the Rorschach, with *how* he approaches his task, although some have attempted to analyze the TAT in these terms. To help us in our analyses of such content, we have drawn heavily on psychoanalysis, the one system of content psychology which, isolated in the clinic, survived the mass attack of behaviorism in the laboratory. Murray in his original system of analysis for the TAT attempted to provide us with a much broader vocabulary for the analysis of content, but, by and large, in our analyses we draw upon relatively few

general psychoanalytical concepts such as sex, aggression, parent-child relationships, and the like. This, to my way of thinking, is an impoverished set of concepts for dealing with mental content, but it is nonetheless the one real and vital one in the United States today.

What evidence is there that this tendency to concern oneself with mental content is of growing influence? In the first place, we must not underestimate the conservative resistance to the belief that such a psychology is possible. Even Freud's generalizations about the importance of certain basic conflicts such as those involved in the Oedipus complex are under constant attack. To some extent the attacks are motivated by the conviction that generalizations about content are really impossible. The argument runs that there are no general concepts which can serve to describe the human situation at *any* place or time in history. What about cultural relativism? After all, individuals differ widely in what they think and so do cultures. Some have an Oedipus complex and some do not. How can we generalize about anything except the process by which individuals arrive at their ideas? The ideas themselves are completely relative. One can perhaps be literary about them, but not scientific. This is the argument and there is no answer to it, except to prove that it can be done fruitfully. Many of us are convinced already, for example, that despite individual and cultural variations, it is a major scientific achievement to have focused attention on the framework of the mother-son relationship as of primary importance in the development of the individual, and to have worked out some of the taxonomy of this and the allied relationship with the father.

Meanwhile, there have been some new developments which would encourage us to believe that perhaps a psychology of content is possible. Take the re-

search report on *The Authoritarian Personality* (1), for example. I would contend that the essential issues raised by this research are issues in the psychology of content. It represents to some extent a fusion of psychoanalytic structural concepts with certain concepts drawn from political ideology. Whether we like the fusion or not does not really matter too much. From the methodological point of view it represents an exciting step forward since its authors have drawn on political ideology as well as psychoanalysis to help explain the structure of personality. When our science has matured to the point where we can draw not only on political ideology, but on economic, religious, esthetic ideologies, and the like, then we will be on the way toward developing a really fullblown psychology of mental content.

Our own research on *The Achievement Motive* (2) has contributed as much to these conclusions as anything else. We started with the relatively simple task of identifying those types of imagery in a TAT-type record which indicated the presence of a motive to achieve or succeed. After we were able to identify reliably this item of mental content, we were able to select individuals whose thought processes contained a lot of such items and other individuals whose thought processes contained few such items. We were then faced with the question of how these people differed. Do they behave differently? Yes, they do. The ones with a lot of achievement imagery tend to learn faster, to perform better, to set different levels of aspiration, to have a better memory for incompleted tasks, to perceive the world in different terms, etc. Perhaps even more interesting was the question of how they got that way. How is it that some people tend to think more often in achievement terms? We were led back to the mother-son relationship and found that independence training seems to be associated with achievement motivation. That is, those mothers who encouraged their sons to develop independently, to learn their way around by themselves, seemed to have sons with higher achievement motives. But we pushed the question one step further back. How is it that some mothers favor independence training more than others? This raised the question of values, and values raised the question of religious ideology. Then we found that attitudes toward independence training were not randomly distributed through various population subgroups. Instead, Protestant and Jewish parents were much more likely to favor early independence training than were Catholic parents and this, in turn, seemed to fit into the belief systems and emphases of these three religions (3). And if this is so, we can begin to trace some of the details of the connection between Protestantism and the rise of capitalism as originally outlined by Max Weber (5) and R. H. Tawney (4).

So our recent research has led us into the relationships between religious values, independence training, achievement motivation, and economic development. We think we are beginning to discover some connections among these phenomena which can be traced out with a fair degree of scientific confidence. But whether we succeed or not, the point I am trying to make is that by concentrating on one item of mental content, namely achievement imagery, we have opened up a whole new set of problems in social science that can be investigated by psychology.

Now let us pause a minute and try to reconsider what has happened. The "new look" in the study of mental content really involves neither introspection nor projective testing in their pure forms. Instead I prefer to call it "thought sampling," and to use the analogy of the "blood count" from the medical laboratory to explain what I

have in mind. Just as we need a sample of blood to make a white cell count, we want a sample of thoughts or ideas to make our imagery counts. In general, we get these samples by asking the subject to write stories to certain cues, usually verbal or visual. Having gotten our thought sample, we have to learn to recognize certain types of imagery, just as the medical technician has to learn to recognize a white blood cell when he sees one. This involves a great deal of preliminary work so that we can define the characteristics of the imagery carefully and then train individuals to recognize what they are looking for. It does require training, but probably no more training than a medical technician needs to be able to distinguish one type of blood cell from another. That is, it does not involve high-level judgment, but is essentially a "pointing" operation which is a little, but not much, more difficult to make than pointing to an animal's right turn in a maze. Watson need not have feared for the objectivity of this kind of analysis. The record is permanent. The same person can look at it again and again, or several people can analyze it. It is quite possible to get a reliable and objective result.

If we put the "new look" in mental content in these general terms, it is immediately clear that we have a number of problems to solve. For example, there is the sampling problem. Under what conditions should we get our thought samples? What cues should we use? Should we get long samples or short samples? What about the subject's set? How does it influence content? Here it becomes obvious that traditional projective testing elicits only a very small segment of the possible types of mental content. To the extent that we stay within the limited framework of the traditional TAT cards, for example, we are bound to have a biased sample of what goes on in a man's mind.

An even more important problem has to do with the decision as to what categories for content analysis we use. This is the heart of the problem, since we will get theoretically meaningful results, or generalizations that hold for a wide variety of situations, only if we choose the right categories to begin with. How does one discover the right categories? The literature of social science is strewn with content analyses of everything from open-ended interviews to "soap operas"—analyses which are purely *ad hoc*—for the immediate practical purpose in hand. I am certainly not arguing for more of this industrious busy work. The categories must be meaningful; they must be related to theory; they must be trans-situational—i.e., applicable to more situations than the one to which they are first applied. It takes inspiration or luck or hard work or something to discover such a category, just as it did in biology to discover what was the most useful of many possible ways to classify blood cells. The only concrete suggestion I have as to how to proceed, which comes from our own experience, is to choose those categories which show significant shifts as a result of experimental operations. Whether this is an unnecessarily restrictive rule, I do not know. At any rate it certainly eliminates many possible categories, and it seems to be roughly the one which the chemists have used in setting up the classification of elements.

This brings us back to Titchener. Looking back with the perspective of history, we can now see that Titchener's structural psychology failed for two reasons. In the first place, the content categories he chose did not turn out to be fruitful. They were not related to experimental operations on the one hand or to other types of behavior on the other. For these or other reasons, they simply did not lead to theoretical development. Therefore they were the

wrong categories. In the second place, and this is of major methodological importance, his students categorized their own data. The essence of introspection is that the same person serves both as a source of data and as a categorizer of them. This has an obvious weakness, a weakness which has been perpetuated in self-descriptive personality inventories. It is simply that the subject may have a very imperfect or incorrect idea of what categories his thoughts belong in. This may be because his categories are different from the ones we as scientists want to use or because he may really misperceive himself. The great contribution that both Freud and the projective testing movement made was that neither asked the subject to pass judgment on his thoughts as they appeared to him. Both simply asked for a sample of those thoughts and then left the categorization process to an outside observer. This was an important methodological advance, the significance of which I think we are only just beginning to appreciate.

If psychologists are to re-enter the field of mental content and start classifying it according to categories of genuine theoretical fruitfulness, I fear they will have to return to disciplines they have long neglected. In the twenties, in the heyday of behaviorism, we were proud that we knew nothing of religion, of art, of history, of economics, or politics (except in a personal, often naive way). We didn't need to know about these things if we were only interested in process variables. We could make our own choice of a task situation—for example, the rat in a maze—and what we found out there about how the rat learned the maze would apply equally well to all (including human) learning situations. We could afford to be ignorant of many things that man has thought about. But if the psychology of content develops as I think it will,

we shall have to go back to getting a broad, general education. Certainly nothing in my training *as a psychologist* prepared me to handle problems in religious belief systems or economic development. Yet these are typical of the problems which I think will begin to arise increasingly often in the new psychology of content, and we simply cannot afford to be naive and pretend that research scholars in these fields have nothing to tell us.

If my analysis is correct, we are on the brink of an important new development in psychology. Because of methodological improvements, we are about to take up again some of the problems in mental content that formerly were considered to be an essential part of psychology. And it is my conviction that the projective testing movement is to be thanked for keeping an interest in content alive in an era when most theoretical psychologists were otherwise occupied, and for providing us with the methodological advance that enabled us to escape from the blind alley into which introspection had led us.

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ON MAKING PREDICTIONS FROM HULL'S THEORY

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The theory of behavior proposed by Hull in *Principles of Behavior* (5) and modified several times thereafter (6, 7, 8) has called forth numerous experimental studies and theoretical articles focusing upon the empirical plausibility of its postulates. The present paper is not concerned with this question of plausibility. Rather, we wish to inquire whether the theory as stated leads logically to predictions which may be tested empirically and, if so, whether these predictions include any sizable proportion of the theorems commonly supposed to follow from the theory. We will conclude that the answer to the first question must be "No," except under special circumstances, and that the answer to the second question is also "No." These answers will result from application of a strict interpretation of the theory, an interpretation which may seem unduly restrictive to some theorists. It should be noted, however, that only a highly developed theory such as that of Hull could be subjected to an analysis of the type to be presented. The reader must judge for himself whether the conclusions reached in this inquiry are reasonable.

Our remarks will have primary reference to *Principles of Behavior* because it is the most widely known presentation of the theory. The conclusions reached would also result from consideration of any of the revised postulate systems which Hull proposed.

The first impression of a person reading Hull's system or the body of literature surrounding it might well be that a vast number of predictions from the theory have been made and tested. Because a considerable amount of data was presented and discussed in Hull's formulation of his theory, it is tempting to conclude that this material at least has already been shown to be consistent with the theory. In fact no one has publicly questioned the presumption that the empirical data cited in *Principles of Behavior* is embraced by the theory. Yet this presumption is false. There was, to be sure, a romance between theory and fact, but the wedding did not take place. Hull never quite compared the implications of his theory with the data he discussed.

AN EXAMPLE OF THE PREDICTIVE DEFICIENCIES OF HULL'S THEORY

To illustrate the respects in which Hull's theory fails to make predictions consistent with the data presented when the theory was first formulated, let us consider a particular experiment. This experiment, the first discussed in Hull's development of the construct called *habit strength* ($_sH_R$), is Hovland's (4) study of the relationship between the number of reinforced trials (N) and the amplitude (A) of the galvanic skin reaction. Hull summarized Hovland's results with the following equation, which is (except for notation) the former's equation 4:

$$\mu_A = 14.1(1 - 10^{-.033N}) + 3.1 \quad [1]$$

where μ_A is the population mean amplitude of response estimated from Hovland's data. Equation 1, then, is a statement of an empirical law which Hull's theory might reasonably be expected to encompass. We will require of the theory that it yield equation 1 as a theorem, or at least that it yield a theorem consistent with the data upon which this equation was based. Since mathematical statements of the postulates are presented by Hull, it will be possible to make mathematical deductions in order to permit comparison of equation 1 with a theorem stated in mathematical form.

Finding the value of $_sH_R$. The first step in the generation of a theorem relating to μ_A to N is to tie N, an antecedent condition, to Hull's intervening variables. Then the intervening variables may be tied to μ_A, a consequent condition, and the prediction will be complete. Accordingly, we now consider $_sH_R$, the first intervening variable involved in the predictive chain. On the basis of the Hovland experiment and others, Hull defined $_sH_R$ by his equation 16, the mathematical statement of his Postulate 4. We present an equivalent definition, Hull's equation 26:

$$_sH_R = m(1 - 10^{-iN}) \qquad [2]$$

where m is the maximum habit strength obtainable under any given experimental situation and i is a constant reflecting the rate of approach of $_sH_R$ to m. Hull's equation 26 is used here in the interest of simplicity: his equation 16 has three factors whose values depend upon experimental variables held constant by Hovland. Therefore all three factors may be subsumed under a constant value of m.

Finding the value of $_s\bar{H}_R$. The second intervening variable to be con-

sidered is *effective habit strength ($_s\bar{H}_R$)*. Hull's equation 29, the mathematical statement of his Postulate 5, may be combined with our equation 2 to yield the following expression:

$$_s\bar{H}_R = m(1 - 10^{-iN}) \qquad [3]$$

if we presume that no difference existed in stimulation at the time of training and the time of testing in Hovland's experiment.

Finding the value of $_{s_i+s_D}\bar{H}_R$. Now it becomes necessary to relate N to the total effective habit strength ($_{s_i+s_D}\bar{H}_R$), by combining $_s\bar{H}_R$ with the effective habit strength loading of the drive stimulus ($_{s_D}\bar{H}_R$). First we infer that $_{s_D}\bar{H}_R = {}_s\bar{H}_R$ on the basis of a comparison of columns 3 and 8 of Table 5 of *Principles of Behavior*. Then we employ our equation 3 together with Hull's equation 30, the mathematical statement of Major Corollary I, in the special form given by the seventh equation on page 255, to establish the relationship between $_{s_i+s_D}\bar{H}_R$ and N:

$$_{s_i+s_D}\bar{H}_R = m[1 - 10^{-iN}]$$
$$\times \left[2 - \frac{m(1 - 10^{-iN})}{100} \right]. \qquad [4]$$

Finding the value of $_sE_R$. The process of deduction must now be continued with the specification of the manner in which reaction potentiality ($_sE_R$) depends upon N. Using Hull's equation 34, the mathematical statement of Major Corollary II, and our equation 4, we obtain the required equation:

$$_sE_R = m[1 - 10^{-iN}]$$
$$\times \left[2 - \frac{m(1 - 10^{-iN})}{100} \right] D' \qquad [5]$$

where D' is our simplification for the expression $(\dot{D} + D)/(\dot{D} + M_D)$, which is a constant for the Hovland experiment. D', then, represents the com-

bined magnitude of all drive effects in Hovland's experiment.

Finding the value of $\dot{I}_R$. The development of a predictive equation relating μ_A and N now involves the determination of the relationship between the amount of net inhibition ($\dot{I}_R$) and N. At this point Hull's Postulates 8 and 9 must be considered. We have no choice except to conclude that these postulates, as stated, provide no basis for a specification of the relationship desired. Although Postulate 9 states a rule for summating reactive inhibition (I_R) and conditioned inhibition ($_sI_R$) to obtain $\dot{I}_R$, neither postulate gives much assistance in determining the values of I_R and $_sI_R$.

The logical errors of Postulates 8 and 9 are as follows: (*a*) Equation 36, the mathematical statement of Postulate 8a, contradicts equation 42, the mathematical statement of Postulate 9a, except when $_sI_R = 0$. (*b*) In equation 37, the mathematical statement of Postulate 8b, the symbol *n* is used to represent the number of reaction evocations producing inhibition. Since *n* is employed on the same page to represent the number of unreinforced reaction evocations required to produce extinction, this usage contradicts the verbal statement in Postulate 8 which asserts that, *whenever* a reaction is evoked, inhibition will be created. (*c*) The verbal Postulates 8b and 8c refer to $\dot{I}_R$, whereas the mathematical statements of those postulates refer to $_sI_R$. (*d*) If Postulates 8b and 8c are taken to refer to $_sI_R$ in order to provide an independent definition of one component of $\dot{I}_R$, then I_R is still left without a definition, and vice versa. (*e*) The symbol I_R in equation 41, the mathematical statement of Postulate 9a, must be replaced by $t'''I_R$, the symbol for the amount of I_R remaining after inhibition has been allowed

to dissipate for time t'''. Otherwise equation 40, the mathematical statement of Postulate 9d, can play no part in the prediction of behavior under any circumstance to which the theory may be applied.

Most of the inconsistencies and omissions just noted have previously been discussed either by Koch (**10**) or by Montgomery (**12**). These inadequacies of the theory may be removed by rewriting and expanding Postulates 8 and 9. Several possible modifications could be proposed, either on theoretical or empirical grounds. However, we present the revisions which seem most in keeping with Hull's intentions when writing these postulates. Only the mathematical statements of the revised postulates need be given here:

Revised Postulate 8a:

$$_s\bar{E}_R = {}_sE_R - \dot{I}_R. \qquad [6]$$

Revised Postulates 8b and 8c:

$$I_R = (\dot{c}N')/(B - W). \qquad [7]$$

Revised Postulate 9b:

$$\dot{I}_R = t'''I_R + {}_sI_R - \frac{(t'''I_R)\,({}_sI_R)}{100}. \qquad [8]$$

Postulate 9c:

$$_sI_R = I_R. \qquad [9]$$

The above postulates are for the most part self-explanatory. It will be noted that we have only one equation for Postulates 8b and 8c, and that N' is used in place of *n*. Only one equation is necessary because Hull's mathematical statements of Postulates 8b and 8c are algebraically equivalent. The symbol N' should be taken to refer to the number of reaction evocations observed either during acquisition or extinction or a habit, or during both acquisition and extinction. In Hovland's experiment

$N' = N$ because testing followed N reaction evocations.

Since Postulates 8d and 9b require no modification, they have not been presented above. Postulate 9c will be seen to be completely new to the system. We have no particular faith in its adequacy, but introduce it only because some postulate about sI_R is essential if predictions are to be made within a theory containing Postulate 9a.

The modifications of Postulates 8 and 9 proposed above permit us to derive an equation relating $\dot{I}_R$ to N, as was our original purpose in this section. Since t''' was constant in Hovland's experiment, we replace the term $e^{-qt'''}$ by the constant k and write the following equation based upon Postulates 8 and 9 in their revised form:

$$\dot{I}_R = \frac{cN}{(B - W)}$$
$$\times \left[1 + k - \frac{ckN}{100(B - W)}\right]. \quad [10]$$

Finding the value of $s\bar{E}_R$. A specification of the relationship between Hull's next construct, effective reaction potential $(s\bar{E}_R)$, and N may be obtained by substituting the quantities from the right-hand sides of equations 5 and 10 into equation 6 to yield the expression:

$$s\bar{E}_R = m[1 - 10^{-iN}]$$
$$\times \left[2 - \frac{m(1 - 10^{-iN})}{100}\right]D'$$
$$- \frac{cN}{(B - W)}$$
$$\times \left[1 + k - \frac{ckN}{(100)(B - W)}\right]. \quad [11]$$

Finding the value of $s\dot{E}_R$. One last intervening variable remains to be defined in terms of N before a prediction of amplitude as a function of N becomes possible. This construct,

momentary effective reaction potential $(s\dot{E}_R)$, is defined by Hull's equation 44, the mathematical statement of his Postulate 10:

$$s\dot{E}_R = s\bar{E}_R - sO'_R \quad [12]$$

where $sO'_R = sO_R$ when $s\bar{E}_R \geq sO_R$ and where $sO'_R = s\bar{E}_R$ when $s\bar{E}_R < sO_R$, the range of sO_R being specified by the relationship $0 \leq sO_R \leq 6\sigma$. The distribution function of sO_R is that of a normal variable truncated three σ's above and three σ's below its mean of 3σ:

$$f(sO_R)$$
$$= \frac{1}{.9973\sqrt{2\pi}\sigma} e^{-\frac{1}{2}\left(\frac{sO_R - 3\sigma}{\sigma}\right)^2}. \quad [13]$$

Substituting the value of $s\bar{E}_R$ from equation 11 into equation 12, we obtain the desired relation between $s\dot{E}_R$ and N:

$$s\dot{E}_R = m[1 - 10^{-iN}]$$
$$\times \left[2 - \frac{m(1 - 10^{-iN})}{100}\right]D'$$
$$- \frac{cN}{(B - W)}$$
$$\times \left[1 + k - \frac{ckN}{100(B - W)}\right] - sO'_R. \quad [14]$$

Predicting A from N. Given equation 14, it is now possible to use one final postulate of Hull's system to predict the value of the response measure A as a function of N. Substituting from equation 14 into Hull's equation 50, the mathematical statement of his Postulate 15, we obtain:

$$A = h'\left\{m[1 - 10^{-iN}]\right.$$
$$\times \left[2 - \frac{m(1 - 10^{-iN})}{100}\right]D'$$
$$- \frac{cN}{(B - W)}\left[1 + k\right.$$
$$\left. - \frac{ckN}{100(B - W)}\right] - sO'_R\right\} - i'. \quad [15]$$

Equation 15 would be completely acceptable for predictive use if it were not that $_sO'_R$ is a random variable. Consequently A can be estimated from equation 15 once the parameters of that equation are known, but the error of prediction will sometimes be as large as the range of possible $_sO'_R$ values, i.e., up to six σ's. For this reason we would customarily not attempt to predict A. Rather we turn our attention to the prediction of a measure of central tendency of the A distribution.

Predicting μ_A from N. All the common measures of central tendency could be determined for the A distribution. Since the Hovland experiment was a study of the mean alone, we consider only μ_A. Two problems arise at this point: (*a*) Since Hovland employed several different subjects in his experiment, we must ask whether the different persons should be presumed to have different values for the constants in equation 15. (*b*) We must determine the mean of an $_sO'_R$ distribution which is always truncated and in addition sometimes has a discrete probability density at the point $_sO'_R = _s\bar{E}_R$.

The first problem raised is so complex that it must be treated in detail later in this paper. In the interests of simplicity, we assume for the present that all subjects given N reinforced trials will have identical $_s\bar{E}_R$ values, identical $_sO'_R$ distributions, identical h' values, and identical i' values. In this case the population mean amplitude of response, μ_A, is equal to h' times the population mean of the bracketed term in equation 15 less i'. Since the only random variable in this bracketed term is $_sO'_R$, the determination of μ_A depends primarily upon the solution of problem b above.

We present without proof the equations for $\mu_{sO'_R}$:

$$\mu_{sO'_R} = 3\sigma \quad \text{when} \quad _s\bar{E}_R \geq 6\sigma \quad [16a]$$

and

$$\mu_{sO'_R} = _s\bar{E}_R \int_{_s\bar{E}_R}^{6\sigma} f(_sO_R) d_sO_R + \int_0^{_s\bar{E}_R} {_sO_R} f(_sO_R) \, d_sO_R$$

$$\text{when} \quad _s\bar{E}_R < 6\sigma. \quad [16b]$$

On the basis of equations 16a and 16b, the bracketed term of equation 15 has a known mean, and the value of μ_A becomes:

$$\mu_A = h'\left\{ m[1 - 10^{-iN}] \right.$$
$$\times \left[2 - \frac{m(1 - 10^{-iN})}{100} \right] D'$$
$$- \frac{cN}{(B-W)}\left[1 + k - \frac{ckN}{100(B-W)} \right]$$
$$\left. - 3\sigma \right\} - i' \quad [17a]$$

$$\text{when} \quad _s\bar{E}_R \geq 6\sigma$$

and

$$\mu_A = h'\left\{ m[1 - 10^{-iN}] \right.$$
$$\times \left[2 - \frac{m(1 - 10^{-iN})}{100} \right] D'$$
$$- \frac{cN}{(B-W)}\left[1 + k - \frac{ckN}{100(B-W)} \right]$$
$$- {_s\bar{E}_R} \int_{_s\bar{E}_R}^{6\sigma} f(_sO_R) \, d_sO_R$$
$$\left. - \int_0^{_s\bar{E}_R} {_sO_R} f(_sO_R) \, d_sO_R \right\} - i' \quad [17b]$$

$$\text{when} \quad _s\bar{E}_R < 6\sigma. \quad [17b]$$

How the prediction and the data compare. Equations 17a and 17b are the culmination of the process of predicting the functional relationship between μ_A and N from Hull's postulates and a consideration of the experimental design employed by Hovland. Equation 17a can be compressed into an expression suitable for curve fitting, and will then contain six constants. It will have terms involving N to the

first and second degree as well as terms involving 10^{-4N} and 10^{-2iN}. Equation 17b cannot be employed at all unless a special assumption is made regarding the value of $_s\bar{E}_R$ relative to σ at some value of N. Furthermore, one cannot tell when to use equation 17a in preference to equation 17b until this assumption is made. Thus we know little more than that predictions could be made with equation 17a if N were greater than some unknown value.

Under the presumption that this required N is quite small, we compare equation 17a with Hovland's findings as a means of testing the predictive utility of Hull's theory. The result of this comparison is not favorable: equation 1, the result of Hull's curve fitting of the empirical relationship observed by Hovland, bears almost no resemblance to equation 17a. Since equation 1 cannot be generated from the theory, it must be considered an empirical equation only.

From the above we conclude that the theory does not meet our requirement that it should yield equation 1 as a theorem. The possibility remains that equation 17a is consistent with the data upon which equation 1 was based rather than with equation 1 itself. However, since Hovland obtained data for only five values of N whereas equation 17a contains six constants, a fitting of his data to that equation would be a trivial accomplishment.

The predictive process just completed need not be taken as conclusive evidence that Hull's theory contradicts any data from which he built that theory. By no means, however, should it be taken as confirming evidence for the theory. If equations 17a and 17b could be fitted to experimental data having more values of N than Hovland's data, the prediction might be considered adequate. Even then, however, the theory as originally stated would be considered objectionable for several reasons: (a) Revision of Postulates 8 and 9 was necessary before any prediction at all could be made. (b) The restrictions imposed upon $_sO'_R$ at the value $_sO_R > _s\bar{E}_R$ cause the derivative of the μ_A function to be discontinuous when $_s\bar{E}_R = 6\sigma$, a prediction which is conceivable but unattractive from an empirical point of view. (c) Special assumptions will have to be made regarding the constants in equation 17a before curve fitting with it can be attempted. (d) Restrictions will have to be placed upon the constants in equation 17b if it is to be a monotonic increasing function of N, as one would expect from existing data.

The foregoing remarks have been designed to show the implications of Hull's theory as it relates to a specific experimental study. Comparable conclusions would be reached in the theoretical analysis of any other study discussed in *Principles of Behavior*. In all cases the predictive equations derived from the theory as stated will fail to conform to Hull's empirical equations. The predictive equations will be much too complex, a result of undue complexity in the definition of $_{s_1+s_D}\bar{H}_R$, $\bar{I}_R$, and $_sO_R$. These difficulties could be eliminated by appropriate modification of the theory. This modification will not be attempted here, however, because our present purpose is to consider the properties of the theory as it now exists.

DEGREES OF SPECIFICITY IN MAKING PREDICTIONS FROM A THEORY

The theory in question might be defended by some of its advocates on the ground that we have taken its

postulates too seriously—in short, that it should not be considered a system from which predictions as explicit as those of equations 17a and 17b should be made. The argument, we are sure, would not have been employed by Hull, since he employed almost all conceivable means of mathematizing with his theory and his data, except the approach presented above. It is true, however, that many of Hull's corollaries are stated in qualitative form and that many experiments relating to the theory have been designed to test predictions less involved than the one just stated. We now inquire whether the occurrence of a verification or verifications of predictions of a simple nature can mitigate the dissatisfaction produced by the results presented above, and how predictions of this nature should be made.

The prediction of behavior to be expected in a given experimental situation may be as coarse as predicting from an inequality in magnitude of some experimental variable to a corresponding inequality in some response measure. It may, on the other hand, be as fine as the prediction of the numerical value of some parameter in an equation relating the experimental and response variables. The range of coarseness of predictions which have been made from Hull's theory is illustrated by three experiments, performed by Meehl and Mac-Corquodale (11), Siegel (14), and Burke (1).

The prediction of inequalities. In an attempt to explain latent learning phenomena, Meehl and MacCorquodale (11) state that feeding animals in a box, outside a six-unit Blodgett multiple-T maze to be learned, should arouse a secondary drive state which would increase the over-all magnitude of drive $(\bar{D})$. This, in turn, would be accompanied by an increase in $_s\bar{E}_R$ for animals thus fed as compared with animals not given this feeding. The differential $_s\bar{E}_R$ values for these two groups of animals are then taken to imply that the animals with the greater $\bar{D}$ will show shorter mean running times through the maze, smaller mean Blodgett errors, and a smaller sum of weighted errors than the other group. Each of these predictions was verified experimentally by Meehl and MacCorquodale, thereby supporting their notion that latent learning may be explained in some cases as the result of a modification in drive at the time reward is introduced for the first time.

The prediction of the general form of a relationship. A second level of specificity in prediction is exhibited in the report of an experiment by Siegel (14). This experimenter forced human subjects to make from 0 to 160 pressing responses with a switch on their right side before permitting 10 free choice responses in which either a left or a right switch could be selected on each trial. Using the verbal statement of Hull's Postulate 8b, Siegel stated that the total amount of inhibition $(\dot{I}_R)$ would be a linear function of the number of forced choices (N'). He then predicted that the mean of the proportions (p') of left responses would also be a linear function of N'. Siegel's predictions were verified, thus giving apparent support to Hull's theory.

The prediction of numerical values of parameters. The prediction of exact numerical values of parameters in theoretical equations may be based almost completely upon theoretical considerations, or it may be based upon the determination of its value from previous data, in which case the experimenter attempts to recover that value with new data. The latter

method only will be considered here.

An example of this type of prediction is found in a study by Burke (1), who worked with a somewhat modified version of Hull's theory. For each animal used in his experiment, Burke first fitted a theoretical cumulative distribution function of the asymptotic running times obtained in a straight runway. This provided a test of the theory at the second level of specificity, the comparison of empirical data with equations having a given general form. When the theory proved satisfactory at this level, Burke proceeded to substitute the exact values of parameters from these asymptotic equations into the theoretical equations for preasymptotic trials. Since the latter equations were developed independently of the preasymptotic data which they were designed to describe, this prediction of the magnitude of preasymptotic running times could be compared with the running times actually obtained, to provide a test of the predictive usefulness of the exact numerical parameter values. Because the theoretical equations described the preasymptotic data satisfactorily, the constants taken from the asymptotic data were said to be recovered in the preasymptotic data.

The relative merits of the three types of prediction. With examples of the three levels of specificity before us, we may inquire as to the advantages possessed by each method. First, we will consider the amount of deductive effort required, and then we will ask which methods provide the most severe tests of the theory.

Because a prediction from an inequality in an antecedent variable to an inequality in a consequent variable is seldom accompanied by a long demonstration that the first inequality implies the second, it may appear that this type of prediction involves less deductive labor than do the other types of prediction. In some cases this difference may in fact be a legitimate reflection of a reduction of effort required to make a prediction. In most cases, however, Hull and the other workers making predictions of inequalities have not actually generated their theorems or corollaries by strict application of logical operations to Hull's postulates. Thus Meehl and MacCorquodale's predictions were not developed rigorously, a fact which they implicitly admit by placing quotation marks around the word *derivation* when discussing their hypothesis. Furthermore, at least two of their predictions cannot be rigorously deduced from the theory, because they refer to response measures as yet unrelated to $_s\bar{E}_R$ within the theory.

While we accept the empirical relevance of many studies in which predictions have been made without logical demonstration that they flow from the theory, we hold that their theoretical relevance is problematical until such demonstration is forthcoming. The rough and ready intuitive procedures customarily employed must be replaced either by careful algebraic manipulations of inequalities following the complete chain from $_sH_R$ to $_s\bar{H}_R$ to $_{s_1+s_D}\bar{H}_R$ to $_sE_R$ to $_s\hat{E}_R$ to $_s\bar{E}_R$ to the response measure, or by derivation of equations comparable to equation 17, and this step must be followed by examination of such equations in order to determine whether certain inequalities hold. It might be presumed, for example, that, in experimental conditions like those of Hovland, the theory implies that $N_1 > N_2$ should be accompanied by $\mu_{A_1} > \mu_{A_2}$; but such an inference should not be made simply by referring to Postulate 4, making the ritualistic statement

that all other variables are equal, and jumping to Postulate 15. One acceptable option is to trace the effect of the inequality in N's through the chain of intervening variables listed above to μ_A. A better solution, if both N's are large enough for equation 17a to be applicable, is to determine the difference $\mu_{A_1} - \mu_{A_2}$ by successively substituting N_1 and N_2 into that equation and subtracting. If the resulting difference is in general positive for $N_1 > N_2$, then the inference is correct. Unfortunately, however, we find that the following condition must hold before $N_1 > N_2$ can be said to imply $\mu_{A_1} > \mu_{A_2}$:

$$h'\left\{mD'\left[\left(\frac{m}{50}-2\right)(10^{-iN_1}-10^{-iN_2})\right.\right.$$
$$\left.-\frac{m}{100}(10^{-2iN_1}-10^{-2iN_2})\right]$$
$$-\frac{c(1+k)(N_1-N_2)}{(B-W)}$$
$$\left.+\frac{c^2k(N_1{}^2-N_2{}^2)}{100(B-W)^2}\right\}>0. \qquad [18]$$

Not only must the inference drawn intuitively be amended by imposing the restriction (equation 18), but also the drawing of this inference regarding inequalities requires additional derivations beyond that required to make the prediction of a general relationship specified by equation 17a. Thus the less specific prediction has proved the more laborious in this case.

The foregoing is intended as a warning against the misuse of the method of predicting from inequalities to inequalities. It is not, however, directed against experimental tests of predictions which are logically sound. This type of study has the special advantage that the use of data from only two experimental conditions is sufficient to permit detection of gross failures of the theory. Thus, pre-

suming that Meehl and MacCorquodale had obtained data contrary to their predictions and that their predictions could be shown to follow from the theory, the theory would be considered unsatisfactory at that point.

On the other hand, a theory may be unsatisfactory without its delinquencies becoming apparent when tested with only two groups. For example, had Meehl and MacCorquodale deduced a theorem stating that the decrease in mean latency with increasing amounts of extramaze feeding should be linear, and had then performed a second experiment with three or more conditions, they might have found this theorem to fail to predict their new results whereas their first experiment had confirmed a less specific prediction from the same postulates. To go one step further, if the theory could be used to predict the numerical values of mean latencies as a function of amount of extramaze feeding, a third experiment designed to obtain these numerical values might fail to confirm the theory, even if the two experiments performed to test more coarse predictions had yielded confirmatory evidence.

We conclude this section by stating that, when experimental tests of predictions logically derived from Hull's theory have been reported in the psychological literature, they must be accepted as legitimate tests of the theory regardless of the level of specificity of the predictions involved. The relative merits of the three levels should not be gauged by any supposed differences in ease of prediction, but rather by the severity of the test required and by the type of experimental design and statistical analysis desired. As the degree of specificity of the prediction increases, the theory receives a more stringent test, but the

restrictions upon the experimental design and the statistical analysis are likely to increase.

THE PROBLEM OF INDIVIDUAL DIFFERENCES IN PARAMETER VALUES

Earlier in this paper we assumed that all subjects given identical experimental treatment will have identical values of $_s\bar{E}_R$ and of all other parameters involved in the derivation of equation 17. Although Hull and his co-workers came close to making this assumption in their papers on the quantification of $_sH_R$ and $_sE_R$ (3, 9), he seems finally to have rejected it. His final views on this matter seem best to be represented by the following quotation from *Essentials of Behavior* (7, pp. 115–117): ". . . in the molar law of habit formation, $_sH_R = 1 - 10^{.0305N}$, . . . the value .0305 is a constant. . . . Presumably if a group of organisms strictly comparable in all other respects to those from which this equation was secured were tested on the same behavior form, a value within the sampling range of .0305 would again be secured. But if older or younger or diseased organisms were used, or if a different genetic strain or a different species, *and particularly if single organisms were used* (italics ours), presumably rather different exponential values would result."

As we understand the foregoing statement, Hull believed that the parameter values of his equations were characteristic of a population of comparable animals treated identically but not necessarily characteristic of every (or perhaps any) animal in the population. This position finds support in certain experimental work indicating that individual organisms do differ in their parameter values (1, 2). However, if used in this way, Hull's equations would consist of laws about populations rather than about individuals; and their potential usefulness would be greatly altered.

Two kinds of laws compared. The distinction between laws about populations and laws about individuals deserves emphasis. If a relationship is established between an experimental variable and a response variable for an individual, it is a law about an individual. If a relationship is established between an experimental variable and the mean or some other parameter of the distribution of response measures for a population of organisms, it is a law about a population.

Although laws about populations have an initial attraction to the theorist, particularly a theorist who must defend himself against the claims of the idiographic approach, they have a disadvantage which must be recognized when they are used. Because behavior takes place in individual organisms, any mechanism designed to explain behavior changes must be applicable to individuals. But since both experimental findings (1, 2) and mathematical reasoning (13) indicate that equations based upon group data may differ in form or in parameter values from the equations obtained for individuals, mechanisms inferred from population laws may prove quite unsatisfactory. When there is a discrepancy between these two types of laws, a mechanism based upon population laws may not be appropriate to the description of behavior changes in the individual. For this reason Hull's theory might lose much explanatory value if it were taken to be a system of laws about populations.

On the other hand, a law about a population has considerable actuarial value. Once the parameters of a

population law have been estimated, predictions of a very practical nature may be made. Thus we could maximize the number of crossings of the Warden obstruction apparatus (15) by a group of sexually deprived male rats by employing a 24-hr. deprivation period, regardless of whether this period produces a maximal number of crossings in any individual animal.

Alternative solutions to the problem of individual differences. Clearly, the assumption of equal parameter values for all organisms which formed the basis for equation 16 must be rejected. If we accept Hull's solution and decide to establish laws about populations, equation 16 and subsequent equations may still be employed. We simply take, as the $_sO'_R$ distribution, one of inter-individual differences rather than one of intra-individual differences, and proceed as before. With this solution the theory remains testable with group data, but will suffer from the defect, previously mentioned, that it may not lead to appropriate explanatory mechanisms for the behavior of individual organisms.

The other solution is to continue to treat the $_sO'_R$ distribution as one of intra-individual differences and to establish laws about the behavior of individual organisms. It is to be hoped that similar organisms will exhibit similar laws, but this must be demonstrated experimentally before it may be presumed true. With this solution, data presented for a group as a whole can have little bearing upon the validity of Hull's theory. A theoretical statement of the relationship between the experimental variable and the mean response for the group will be extremely difficult because of the differences in parameter values for the different animals in the group.

A choice between these two methods of resolving our problem is not easy. Perhaps it is not necessary at this time. Research workers may use the theory either with groups or with individuals, provided that the difference between the two usages is recognized.

SUMMARY

A literal interpretation of the postulates presented in Hull's *Principles of Behavior* was shown to lead to predictions markedly different from Hull's empirical equation summarizing an experiment upon which his theory was based. Without clarification and simplification of the theory, it is believed that most predictions rigorously deduced from the theory would contradict or at least differ in form from Hull's empirical equations. A comparable statement could be made for each of the later postulate systems developed by Hull.

Three levels of specificity in prediction from Hull's theory were discussed: First, the prediction of inequalities in response measures from inequalities in experimental variables; second, the prediction of the general form of relationships between variables; and third, the prediction of numerical values for parameters in theoretical equations. The theory was seen to be most severely tested by predictions of the most specific nature. Some of the failures of Hull and others to make predictions consistent with the theory were attributed to the false belief that the deductive procedure is necessarily simplified by making predictions at a low level of specificity.

Finally, the fact that individual organisms differ in their parameter values was recognized. Two ways of theorizing which are consistent with this fact were considered: In one case Hull's equations are taken to hold

for the population as a whole, in which case the theory has predictive usefulness but may lack explanatory value. In the other case, equations are to be established for each organism separately, and will be taken to describe the process of behavior change in the individual, whether or not they lead to predictions of behavior in other individuals.

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